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AN ASCENT OF POPOCATEPETL.

A. E. DOUGLASS.

An expedition, consisting of Mr. W. A. Cogshall and the writer, accompanied part way by Dr. T. J. J. See and his brother, who went for a brief outing in the mountains, left Mexico for the ascent of the great volcano on April 12 of this year. Horses and mules were procured at Amecameca, and the fourteen-mile ride from there to the sulphur-smelting works, at a locality called Tlamacas, at the foot of the mountain, was made in five hours, from 2 to 7 P. M. This is an ascent from 8200 feet (2532 metres) to 12,900 feet (3937 metres), but no one experienced any marked inconvenience from the altitude, although the increasing cold after nightfall became very disagreeable. Very little was eaten by us that night or the next morning, to avoid the indigestion which high elevation is apt to bring on.

We left Tlamacas at 5 A. M., on horseback, and at 6:30 reached Las Cruces, at an altitude of about 14,000 feet, whence we proceeded on foot, as is customary. The path is on the northern slope of the cone, and at the time of our trip was but a short distance east of a large field of ice and snow impossible to traverse. On the entire ascent to the crater no snow was encountered, although in some places there were traces of it under the sand, lending to the latter its solidity, and greatly helping us. Mr. Cogshall reached the crater in $4^h\ 10^m$ from the time he left Las Cruces. I was longer, taking fully $4^h\ 50^m$. Frequent rests were necessary to regain breath and allow the pulse to quiet down. An attempt to climb more quickly brought on a very disagreeable but temporary feeling of exhaustion and inability to climb higher. One should be very careful of this, and, on the first sensation of overdoing, rest until entirely relieved, and then go more slowly.* Timing one's self with a watch, taking twenty or thirty steps, and then resting one minute, is the correct way to make a climb of such magnitude at high altitudes.

The view of the crater is strikingly sudden. When I was approaching it without, however, seeing any signs, my guide, who

* One week after writing the above I tried the ascent of a still higher mountain, and failed to reach the top from disregarding this very caution.

carried the barometer and camera, and was always beside me, kept offering encouragement, and finally pointed to a bit of stone some two hundred feet ahead, and said the crater was there. So it proved. On reaching that spot the slope, without warning, rounded over, became level, and descended precipitously into a huge pit, and the strong smell of sulphur showed its volcanic character. The pit is fully two thousand feet across and probably more than a thousand feet deep. Its sides are perpendicular, exhibiting the non-conforming layers of successive lava streams, with here and there, especially on the near side, boulders and cliffs of basalt. Around the edge at the bottom the heaps of the talus are small, suggesting recent activity, while the floor is of a yellowish tint, showing half a dozen bright yellow spots whence sulphur and vapor are ascending. In the center there is a large pond whose water is a brilliant green in color.

The ridge surrounding the crater is serrated, broken into peaks and hollows. The approach is at the lowest point and is upon a ridge formed by a long curved sweep of volcanic sand. The highest point is nearly opposite, and some six hundred feet higher.

One's honor is vindicated on reaching the crater; but having with us a mercurial barometer, through the kindness of Señor Zendejas, Sub-Director of the Central Meteorological Observatory of Mexico, it became necessary to push on as far as possible toward the highest point. A pole was visible on a small peak of the western wall of the crater, which some one had placed there "because he wanted to,"—"*por gusto*," the guide told us. Without expecting to go beyond the pole, I set out with one guide and reached it in half an hour. Thence the way to the summit was in plain view and was obviously difficult. In the mean time, another guide came to help in the attempt, and, incidentally, to accept an extra fee, and we set out after discarding everything not absolutely necessary.

At first the ascent was not steep, but it was in a measure dangerous, if one did not have a good head for high places. The rock was a soft, crumbling lava, which projected in a series of slightly inclined knife edges out over the crater, presenting as we progressed, on our left hand a precipice of a thousand feet, and on our right a sharp descent into deep ice caves. Passing this, a steep narrow ridge of sand led straight to the summit.

The summit is worthy of the mountain. Although composed of sand, it is hardly three feet across. Twenty feet on one side is the crater, and thirty feet away on the other the slope drops

suddenly into the ice-sheet. To the south a great ridge of rock stands out conspicuously, and can be followed by the eye for thousands of feet downward. Far away to the north Ixtaccihuatl, of nearly equal height, mounts into the sky, and one seems to look across a grand cañon worn in some plateau, whose sole remaining points are these two mountain tops.

The air below was hazy, but above the haze a narrow strip of blue showed where the horizon was,—and how high it seemed in comparison with the nearer regions! At this hour of the previous day, between one and two o'clock of the afternoon, clouds had been upon the summit, so, to avoid being caught, the photographs were quickly taken and the barometer readings made, and we prepared to descend.

The ascent from the entrance of the crater to the summit had taken 1^h 40^m. The descent, owing to the character of the way, required 30^m. After stopping a few minutes on the edge of the crater, we began the great descent at 2:30, and in exactly 30^m reached Las Cruces. From there, still on foot, we arrived at the Ranch at 3:48, finding our companions ready for us with a lunch, which, from exhaustion, we could hardly touch. Mr. Cogshall, always more robust, ate sandwitches for both of us. I drank a few swallows of wine and ate a small piece of sweet chocolate. The chocolate, the wine, a few swallows of beef tea, and a little water were all that passed my lips from the time of leaving Amecameca to the return thither.

At half past four we set out for Amecameca, and reached there in four hours, passing through a steeper and more picturesque road than the day before. We were indeed greteful for the good supper and comfortable beds of its pretty little hotel

The barometer readings at the top of Popocatepetl were 15.897 and 15.864 inches, reduced to freezing (403.8 and 403.0 millimetres, respectively), and the standard thermometer registered a temperature of 32°.7 F. (0°.4 C.) When compared with readings at Mexico at one and two o'clock, they gave an altitude for the mountain of 17,775 feet (5418 metres. This is the result by the Smithsonian Tables of 1893; by certain French Tables of a less recent date the result is 17,721 feet, or 5401 metres).

It seems to the writer that during the winter season very valuable series of meteorological observations might be carried on at the top of this mountain. It would then be decidedly the highest in the northern hemisphere and the second highest station in the world, the highest being on El Misti, at Arequipa, Peru, at an altitude of over 19,000 feet (5800 metres), to whose top, by

the way, the observer regularly ascends on mule-back. It would do for this hemisphere what the El Misti station is doing for the southern.

The ascent is inexpensive, both in time and money. We were absent exactly forty-eight hours from the city of Mexico, and our entire expenses reached only nine dollars (gold) for each man. General Ochoa, residing at the Hotel Iterbide, in Mexico, is the owner of the mountain and of the sulphur deposits in the crater, and we were indebted to his courtesy for a note of recommendation to his manager in Amacameca.

For the physical exertion of climbing, the atmosphere we encountered was particularly well adapted. The sky was hazy, cutting off the fiercest rays of the sun, and the air was quiet, so that we did not suffer from cold. It was also especially favorable for obtaining the altitude; for, so far as could be judged by signs, the air was without irregularities in density between the top of the mountain and the city of Mexico. The sheet of thin cloud was far above the top of the mountain, and there were no cloud levels below it. The wind was light, both in the city and on the mountain, and the absence of direct sunlight prevented any local convectional action which might alter the barometric readings. These facts compensated to some extent for the loss, which we greatly felt, of the marvellous view of the table-land of Mexico.

PROFESSOR CHAMBERLIN ON THE NEBULAR HYPOTHESIS.

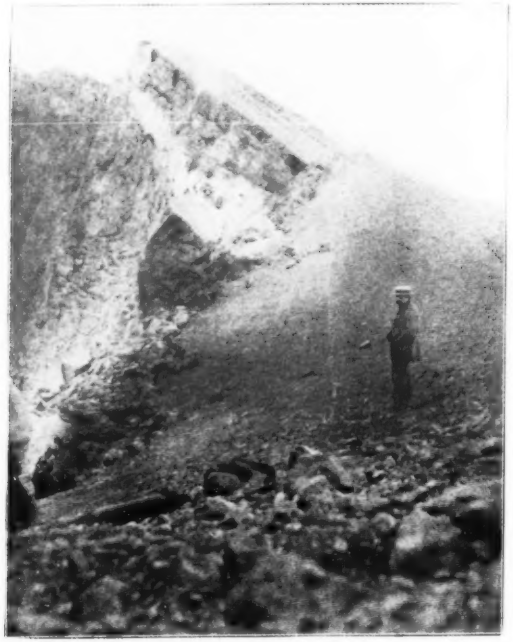
F. R. MOULTON.

For Popular Astronomy.

Professor T. C. Chamberlin, Head Professor of Geology in the University of Chicago, read a paper before the *British Association for the Advancement of Science* at Toronto August 20, 1897, on *A Group of Hypothesis Bearing on Climatic Changes*. It was necessarily somewhat summary but has been published *in extenso* in the October-November issue of the *Journal of Geology*, and proves to be of great interest to astronomers since a large part of it is devoted to subjects which have long been of interest to students of astronomy.

Professor Chamberlin finds that in the earlier periods of geological history the Earth's atmosphere could not have been at all times so extensive and so rich in carbon dioxide as has been very

PLATE XXX.



WESTERN CLIFFS OF CRATER.

ICE FIELD NEAR SUMMIT, POPOCATEPETL.

generally supposed. The salt deposits of Palaezoic times imply a dry atmosphere; and Pleistocene glaciation one possessed of but very little carbon dioxide. On the contrary if the Laplacean nebular hypothesis be accepted and the theory that the earth was once a molten mass, surrounded by a widely extended atmosphere which, cooling and condensing was largely deprived of its carbon, hydrogen and oxygen to form the carbonates, hydroxides and oxides, we should expect to find evidence that the farther back we go the more extensive the atmosphere would become, and the more heavily charged with water vapor and carbon dioxide.

Professor Chamberlin therefore proceeds to raise some doubt about the correctness of the ordinarily accepted nebular hypothesis. He says: "But I venture to question the supposed original molten state. While making no claim to disprove it, I doubt whether it rests on sufficiently solid theoretical grounds to justify the assumptions so unhesitatingly built upon it." His doubts are supported by the following considerations. No phenomenon has been observed which gives decisive support to the gaseous ring theory. It is inconceivable how a ring of such vast radiating surface could have been kept hot during the immense ages required for its condensation to a spheroidal form. A graver source of doubt arises from the high molecular velocities that would be attained under such conditions. From tables which he has given it seems almost certain that even after the mass had attained a somewhat condensed spheroidal form it would have lost all its elements of low molecular weights, such as hydrogen, oxygen, carbon and nitrogen. Their molecular velocities would have become so high that they would have shot off in space with greater than parabolic velocities. The high rate of rotation which the Earth is supposed to have had before the Moon wore it down by tidal friction would only have exaggerated the effects. The very abundant presence of these elements proves that there is a fallacy either in the hypothesis or in the reasoning, and there can scarcely be a doubt that it lies in the former.

Professor Chamberlin then examines the meteoroidal hypothesis and finds it more consistent with geological facts. It is certain that the aggregation of discrete meteorites revolving around the Sun into one mass would be a slow process, and since they would all be revolving in the same direction the kinetic energy of impact would be small, so it is plain that the collection into a body, such as the Earth, might take place without developing at any one time a high temperature. The Earth is growing in this manner now by the acquisition of meteoric matter. As the mass

became of considerable size it would contract, crushing the meteoric stones, liberating the occluded gases and generating heat. If the Earth were supposed to have been homogeneous and to have shrunk from a specific gravity of 3.5 to one of 5.6 which is the average density now, it is found that the process would have generated enough heat to raise the temperature of the whole mass 6560° *C*. This would be amply sufficient to account for all the igneous action known to geologists. In the more unfavorable case of the Moon shrinking from a mean density of 2 to 3.5 (from that of meteoric stones to the Moon's present density) the heat generated would raise the temperature of the whole mass 2900° *C*. This again is sufficient to account for all the phenomena. This hypothesis also explains more satisfactorily the continental elevations, oceanic depressions and the irregular distribution of densities as revealed by pendulum experiments, than does that supposing the Earth to have been originally an extended molten mass.

Professor Chamberlin feels freer under this hypothesis to assume in the different geological ages just such atmospheres as the life and deposits of the time seemed to imply. Dr. Arrhenius has recently demonstrated that the amount of carbon dioxide in the atmosphere has a most important influence upon the climate. A loss of from $\frac{2}{3}$ to $\frac{1}{2}$ of the amount now in the atmosphere would bring on the conditions of Pleistocene glaciation while an increase from 2.5 to 3 times its present amount would produce the mild temperatures of the Tertiary times. The oscillations in climate are ascribed to periodic variations in the amount of carbon dioxide in the atmosphere, the causes of which Professor Chamberlin summarizes as follows:

"The idea hinges (1) on the action of the ocean as a reservoir of carbon dioxide and (2) on the losses of the organic cycle under the influence of cold. Cold water absorbs more carbon dioxide than warm water. As the atmosphere becomes impoverished and the temperature declines, the capacity of the ocean to take up carbon dioxide in solution increases. Instead, therefore, of resupplying the atmosphere in the stress of its impoverishment, the ocean withholds its carbon dioxide to a certain extent, and possibly even turns robber itself by greater absorption, though the diminution of the tension of the carbon dioxide of the atmosphere as its amount is reduced tends to increase the discharge of carbon dioxide from the ocean to restore the equilibrium, and, to the degree of its efficiency which is undetermined, offsets the increased absorption of the cold water. So also, with increased cold the

process of organic decay becomes less active, a greater part of the vegetal and animal matter remains undecomposed, and its carbon is thereby locked up, and hence the loss of carbon dioxide through the organic cycle is increased. The impoverishment of the atmosphere is thus hastened and the epoch of cold is precipitated.

With the spread of glaciation the main crystalline areas, whose alteration is the chief source of depletion, becomes covered and frozen, and the abstraction of carbon dioxide by rock alteration is checked. The supply continuing the same, by hypothesis, re-enrichment begins, and when it has sufficiently advanced warmth returns. With returning warmth, the ocean gives up its carbon dioxide more freely, the accumulated organic products decay and add their contribution of carbonic acid, and the re-enrichment is accelerated and interglacial mildness hastened.

With the re-exposure of the crystalline areas, alteration of the rocks is renewed and depletion re-established and a new cycle inaugurated. And so the process is presumed to continue until a change in the general topographic conditions determines a cessation."

These results lead us to hope that on the ground common to geology and astronomy, as has been the case with chemistry and physics, many discoveries may be made which shall shed a new light on both.

STELLAR BANDS IN THE ZODIAC FROM GEMINI TO SCORPIO.

A. E. DOUGLASS.

For Popular Astronomy.

Various observers interested in the study of the zodiacal light, among whom may be mentioned Professors Simon Newcomb and Arthur Searle, have urged the especial study of stellar light in the zodiac, so that due allowance may be given it in determining contours of the zodiacal band and the Gegenschein. Observations of that class were made by the writer while in South America upon faint branches or large areas of the Milky Way, forty-five degrees or more from the galactic equator, bringing the Magellanic clouds within its reach. The accompanying map represents a more recent attempt to delineate such bands or areas of light in the portions of the zodiac between Gemini and Scorpio.

The drawings were made in March, April and May, 1895. Pro-

fessor Searle's method of observation was used, consisting in contours of equal brightness drawn upon tracing cloth placed over suitable star maps. Heis's Zodiacal Atlas has been found very convenient for this purpose. Twenty-one drawings on sixteen different nights were made, and of these the work on April 16 and 17 is the most complete since on each of those dates some hours were especially devoted to it. The longitudes in which bands were drawn in the various observations, are as follows:

STELLAR BANDS OBSERVED IN 1895.

Date	Observation.	Longitudes.	Date.	Observation.	Longitudes.
		° °			° °
March 24		180-200	April 17	I	170-220
27		160-200	17	II	110-210
30		160-200	17	III	110-240
30	I	180-200	18		150-210
31	II	180-210	19		180-220
April 1		180-200	20		180-200
14		150-200	22		100-180
15		140-220	26		210-230
16	I	90-180	27		210-230
16	III	140-200	May 24		220-240
16	IV	110-220			

All were made at Flagstaff save the final observation, in the City of Mexico. In anticipation of the present publication, independent drawings have been made here within the last few days, which are entirely concordant with these observations in 1895 in nearly every detail. The only correction of importance occurs in the region between the "Sickle" of Leo and the Milky Way, which should be largely filled with light, forming a border to the main stream of the Milky Way. This border extends over a part of the "Great Dipper."

As these bands are near the limit of vision they are exceedingly faint, and of course most reliance is to be placed upon those which show most conspicuously in the diagram; in the fainter portions it is likely that there are some small errors. Their average brightness is about that of the fainter parts of the zodiacal band when observed under favorable conditions. The brightest portion is the north-and-south branch in right ascension 190°. A more nearly circular area in right ascension 165° to 175° is almost as bright and is very closely the same as the average brightness of the Gegenschein. It has occasionaly been mistaken for it and observations on the Gegenschein while in that region are of little value. A small mass of light in right ascension 194° to 196° has also caused an erroneous position to be assigned to the Gegenschein.

An attempt was made to get rid of the effect of the zodiacal

band in this chart but the long continuous band from Praesepe to Scorpio may partly be due to lack of success in this respect. It might be expected that the mere presence of bright stars would interfere with this work, and in some cases cause the appearance of a fictitious band, but in these observations there is no marked connection between bright stars and bands and therefore probably all of these are due to an extended area of fainter light and may legitimately be called branches of the Milky Way. In any case they are illuminations in the sky which may affect observatians on the Gegenschein and zodiacal light.

For reproduction a combination drawing was made on tracing cloth and placed upon the proper map in Heis' atlas (epoch 1855) and a negative made through this.

Since reading proof of all that precedes, I have been fortunate in finding that Professor A. Searle, in 1885, described a band of light passing from Praesepe to β Virginis and thence northward. The western part of this be found attributale to Durchmusterung stars but the eastern portion, particularly the mass of light near right ascension 170°, could not be explained in this way. He suggests diffused nebulosity in that part of the sky.

LOWELL OBSERVATORY, Flagstaff, Arizona.

January 1, 1898.

ON SOME MODERN ATTEMPTS TO REPLACE NEWTON'S LAW OF ATTRACTION BY OTHER LAWS.

KURT LAVES.

FOR POPULAR ASTRONOMY.

Certain phenomena in the motions of some of our planets have led astronomers to question the correctness of Newton's law of gravitation as a general law of nature. It is the purpose of this paper. first to point out those phenomena which cannot be explained from Newton's law, and second to give a short outline of the nature of those laws which have been proposed to replace that of Newton's.

After Laplace and the great contemporary astronomers had accomplished the task of developing the theories of the planets' orbits, LeVerrier undertook the immense work of constructing tables for the great planets based upon all the observations available up to the middle of our century. These tables form the main body of the "Annales de l'Observatoire impérial de Paris" that famous standard work of astronomy. It is known that six quan-

tities—the so-called elements—characterize the motion of a planet about the Sun. If only one planet and the Sun existed these elements would be constant. But since the solar system is made up of a number of planets the effect is that on account of mutual attractions these elements must be made to change with the time in order to represent the disturbed motion of each planet. In the analytical expressions for the elements there occur terms which are of the nature: a constant multiplied by the time t. These terms are called secular terms since they become very sensible when we assign to t values equal or larger than 100 years. To give an example, we choose the orbit of Mercury. One of the elements is the longitude of the perihelion which we will designate with Π. This would be a constant for all time if no planet existed besides Mercury, but as this is not so Π is made to vary with t, so that—always considering but secular terms—it will be increased according to the theory in one century by 527″. This value is calculated from LeVerrier's tables. If we now derive the values of Π for two epochs which are one hundred years distant from each other from *actual observations* we find that the two values differ from each other not by 527″ but by about 568″. This difference of 41″ between theory and observation cannot be explained in any way when we start from Newton's law of general attraction. Le Verrier's attempt to explain it from the perturbing action of a group of intra-mercurial planetoids cannot be maintained since the most systematic and persistent observations made for the purpose of discovering such planetoids have shown no evidence of them. Professor Simon Newcomb has corroborated Le Verrier's work on Mercury by employing the large body of the modern observations since 1846; he shows that the secular acceleration of Π of Mercury which Le Verrier had determined to be 38″ should be increased by 3″ [see Professor Newcomb's paper: The elements of the four inner Planets and the fundamental Constants of Astronomy, Washington 1895, and chapter 28 and 29 of the fourth volume of F. Tisserand's *Mécanique Céleste*] Professor Newcomb proves that the acceleration in question cannot be accounted for by the assumption of a flattening of the body of the Sun since this would not produce a larger effect than 1″.2 in one century.

The example of Mercury was chosen purposely since it represents the most prominent discrepancy between observation and theory (Newton's law). The other differences of the same nature which have been brought to light are those in the longitude of the perihelion of Mars, the longitude of the nodes of Venus

and the famous secular acceleration of the motion of the Moon. All of these irregularities are much smaller than that in the element of Mercury.

We now pass over to a short presentation of the nature of those laws which have been proposed to take the place of Newton's law and see if they will be able to account for these differences.

Professor Asaph Hall has published in the fourteenth volume of the *Astronomical Journal* a paper in which he shows that we could account for the acceleration in the longitude of the perihelion of Mercury by changing in the expression $\frac{fm\,m^1}{r^2}$ of Newton (f being a constant, r the distance between the two masses m and m^1) the power 2 of r into 2.000000151.

With this modification of Newton's law: $\frac{fm\,m^1}{r^{2.000000151}}$ he finds that the perihelion of all the planets will be accelerated in a century in the following manner:

Mercury	41″	Earth	10″
Venus	16	Mars	5

The values for Venus and Earth would not count against this law since the eccentricities of their orbits are very small, but in the theory of the Moon the agreement between theory and observation would be spoiled. Besides there is no doubt that astronomers and physicists would be extremely slow in accepting a law which is made just to fit our present knowledge of existing discrepancies for a general law of gravitation in the manner indicated.

The other laws that have been proposed deserve more attention. They all have this in common that they consist of two parts, of which the first is the expression of Newton's law; the second, an additional term depending upon the time. These laws have been constructed in electro-dynamical theories and bear the name of their inventors. They are the laws of Gauss, Weber, Riemann and Clausius. We need not discuss Gauss's law since it is not in accord with a fundamental principal of dynamics and besides it is not possible to explain by it certain fundamental phenomena in the theory of electro-dynamics.

Weber's law is expressed by

$$\frac{fm\,m^1}{r^2}\left\{1-\frac{1}{c^2}\left(\frac{dr}{dt}\right)^2+\frac{2}{c^2}\,r\,\frac{d^2r}{dt^2}\right\}$$

the additional term added to Newton's expression is

Therefore $$\frac{fm\,m^1}{r^2}\left\{\frac{2}{c^2}\,r\frac{d^2r}{dt^2}-\frac{1}{c^2}\left(\frac{dr}{dt}\right)^2\right\}$$

c is a velocity which must be determined from observations. In electro-dynamics Weber assigns to it the constant value of 439450 kilometres per second. Since the additional term is multiplied by $\frac{1}{c^2}$ it is obvious that it will generally be of the order of a very small correction. Starting with this law of Weber we first attempt to solve the problem of two bodies and we find (see F. Tisserand Mécanique Céleste tome IV pages 500-503) that we can integrate rigorously the 3 differential equation of motion of the second order. The longitude of the perihelion of Mercury will not be any more a constant under the law of Weber but will suffer an acceleration of 6″.77 per century if we assume for c the value given above. For an acceleration of 41″ per century we have to assign to c the value of about 178500 km per second.

The law of Riemann allows also of a rigorous integration of the differential equation of motion. It will suffice to say that it is of the same nature as Weber's law. It will produce an acceleration of the longitude of perihelion of Mercury that is nearly twice as large as that produced under Weber's law. The constant c which enters in the same way in this law must have the value of about 250,000 *km* to correspond to the actual acceleration of Mercury's perihelion.

There is left for discussion Clausius' law. But since the difference between the laws of Gauss, Weber and Riemann on the one side and that of Clausius on the other is mainly based upon a difference in the primary electro-dynamical hypothesis as to the nature of galvanic currents it is for us unnecessary to treat Clausius' law separately, since this difference has no bearing on gravitational problems.

It is of interest to notice that Professor Seeliger has discussed the question whether Newton's law of attraction can be regarded as a universal law of gravitation in a series of papers published in the *Astronomische Nachrichten* (numbers 3287, 3292 and 3304) starting from an entirely different point of view than that outlined before.

Professor Seeliger makes the assumption that the total mass of all the matter in the universe is infinite and then proves that if we apply Newton's law to the infinite universe we shall be led to contradictions. We have thus to assume either the one or the other of the following assumptions:

1. The total mass of the universe is infinite and then we cannot regard Newton's law to be the rigorously exact expression for the attractive forces acting in it, or

2. Newton's law is rigorously correct and then we must conclude that infinite parts of the space *cannot* be filled with mass of *finite* density.

It is certainly more logical to ussume the first and regard Newton's law as an approximative formula which needs supplementary terms, the nature of which has not yet been ascertained. Professor Seeliger gives an example for such a corrected form of Newton's law by multiplying the expression for it by the factor $e^{-\lambda r}$ where e is the base of natural logarithms and λ is a very small quantity. The expression $\frac{fm\,m^1}{r^2}\,e^{-\lambda r}$ has the property that it will not lead to a contradiction as Newton's law did and it will produce besides an acceleration of the longitude of the perihelion of all the planets compared with the values under Newton's law. For $\lambda = 0.00000038$ the secular acceleration of Mercury will be 41″ the acceleration for the other planets will be as follows:

Venus 29″	Mars 20″	Saturn 8″	Moon 0″.9
Earth 24	Jupiter 11	Neptune 5	

The number of laws thus proposed to take the place of Newton's law is quite large. All of them can be said to contain an arbitrary constant which when properly determined will produce just the acceleration of the longitude of the perihelion of Mercury which is observed. As Maurice Levy has pointed out (*Comptes Rendus* CX pages 545-551) innumerable other laws can be constructed by an algebraic combination of those of Riemann and Weber, all of which can be made to cause the discrepancy in Mercury's orbit to disappear. It is obvious that these investigations have not yet brought out a final result but that they certainly will lead the way for future research in this line. In conclusion we might point to the interesting investigations of Professor R. Lehma..n—Filhès which he has published in the *Astronomische Nachrichten* 2630 and more recently in the Sitzungrberichte der Mathematsch physicalischen Classe der Akademie der Wissenschaften zu München 1895 Heft. III. The author makes the assumption that gravity is not propagated instantaneously but like light and electricity requires time for its propagation. In the first paper he finds that the velocity of propagation per second must be larger than 51850 times the velocity of light, which re-

sult he obtains by applying his theory to the Earth's orbit where the secular acceleration of the longitude is certainly not larger than 2″. In the second paper Professor Lehmann—Filhès makes the attempt to account by his theory for the secular acceleration of the motion of the Moon. But the result is a negative one he finds not an acceleration by a retardation. To keep this sensibly small it would follow that the velocity of propagation of gravity should be more than 20 times as large as the value derived for it from the first paper.

THE UNIVERSITY OF CHICAGO,
1897, Dec. 15th.

A NOTE ON A NEW FORM OF MIRROR FOR A REFLECTING TELESCOPE.

F. L. O. WADSWORTH.

FOR POPULAR ASTRONOMY.

The form of mirror described by Professor Poor at the recent astronomical conferences at the Yerkes Observatory seems to be exciting considerable discussion just now. In the last number of POPULAR ASTRONOMY Professor Schæberle's paper on this subject (*A. J.* No. 419) is quoted in full with the remarks, "The point whether or not such a mirror if it could be made would be good or worthless was not even raised if our memory serves us rightly." "It would hardly seem possible that Mr. Poor in his study of the qualities of such a mirror, should have wholly overlooked so important a fact as that raised by Professor Schæberle . . . . . who severely criticises the proposed form of mirror and declares it worthless."

In justice to Professor Poor and to those who participated in the discussion following the reading of his paper, attention should be called to two or three facts in this connection: (1) in the paper which Professor Poor presented at the conferences he stated that he was then engaged in the question of examining the aberration of the proposed form of mirror; (2) In the discussion which followed the reading of the paper, this point was brought up again and the writer stated that it was his opinion, without having carefully examined the question, that the *irregular* aberration *due to errors in the figuring*, would be less in this form of mirror than in the ordinary form. (3). There was a further discussion of these points between Professor Poor and myself, (at which Professor

Brooks and some others, among them I think Professor Snyder, were present), after the meeting, during which Professor Poor showed me some results which he had already obtained and again stated his intention of investigating the whole question more thoroughly. (4). A short time after his return to Baltimore, Professor Poor sent me a copy of his paper which recently appeared in the *A. J.*, (No. 421, Dec. 14th) and which was in my hands several days before Professor Schæberle's article appeared (*A. J.*, No. 419).

With reference to the conclusion reached by Professor Schæberle, that the form of mirror under discussion is "utterly worthless," it seems necessary to point out first of all that this writer's results for the aberration of this form of mirror for points out of the optical axis, are grossly in error, for the reason that he has neglected the fact that the focal surface is not normal to the optical axis of the mirror, (as in the usual form) but is inclined to it at an angle of 45°. In consequence of this the result that Professor Schæberble has really obtained is the maximum diameter of the cross section of the elliptical cone of light from the mirror at a distance equal, (very nearly), to $f \tan 15'$, away from the real focal point.

At this distance from the focus, the cone of light, in a mirror having a ratio of aperture to focal length of 1:7, as assumed by Professor Schæberle, would itself subtend an angle of nearly 43″; *i. e.* about 90 % of the effect he finds is simply due to his being out of the true focus. The aberration in this form of mirror is, as a matter of fact, only slightly greater than in the ordinary form, (unless the angular aperture θ be excessively large).* It is the large inclination of the field to the optical axis which is the chief drawback to the efficient and convenient use of this mirror in ordinary visual or astrophotographic work.

But leaving these applications out of consideration entirely it seems pertinent to inquire whether Professor Schæberle holds the opinion that stellar spectroscopic and spectrographic research is also "utterly worthless." In this field of work, the question of accuracy or inaccuracy of Professor Schæberle's conclusions as to the character of the images formed at some distance from the optical axis of the mirror may be left out of consideration entirely, for we are here concerned only with a point at the center of the field, where, as Professor Schæberle himself acknowledges, the definition is theoretically perfect, (*i. e.* as ordinarily

* See Professor Poor's paper, equation (3) in the *Astronomical Journal* No. 421.

considered). Considering this fact and considering also that for work of the kind just mentioned, this form of mirror, or rather the form of mounting possible with it, has *very great advantages;* (advantages to which I have refered more at length elsewhere, and which will be recognized at once by those engaged in these lines of research); I must emphatically dissent from the opinion which Professor Schæberle has expressed, so far as the *theoretical* advantages of the form are concerned. I still believe that the fundamental difficulty with this form is the practical one of figuring the surface, although I am not at all sure that this difficulty may not be overcome, perhaps much more easily than at first appears, by proper methods of working. For one I most sincerely hope that Professor Poor may continue his experiments in this direction and carry them to a successful issue.

In connection with the matter of figuring the surface of such a mirror I may perhaps call attention again to point (2) of the second paragraph, since it has not been touched upon by Professor Poor in his paper. In looking up the literature of the subject of aberration of mirrors I find that this very point has been considered by Lord Rayleigh and that his results confirm the correctness of the opinion I expressed at the time of the conferences. His conclusions are sufficiently important and interesting in this connection to justify quoting them in full. He says:* "In the case of perpendicular reflection from mirrors the results of §4 lead to the conclusion that no considerable area of the surface should deviate from truth by more than one eighth of the wave length.

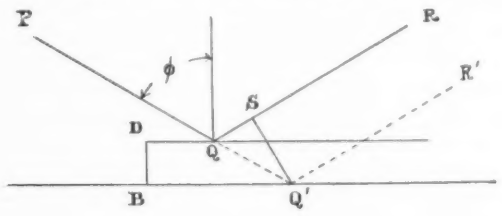


FIG. 7.

"In the case of oblique reflection at an angle, φ, the error of retardation due to an elevation BD (Fig. 7) is

$$QQ' - QS = BD \sec \varphi (1 - \cos SQQ')$$
$$= BD \sec \varphi (1 + \cos 2\varphi) = 2\, BD \cos \varphi$$

* "Investigations in Optics with special reference to the Spectroscopes § 5, On the Accuracy required in Optical Surfaces." Phil. Mag. Dec. 1897, pps. 477-9.

for which it follows that an error of a given magnitude in the figure of a suface is less important in oblique than in perpendicular reflection. At first sight this result appears to be contradicted by experience; for it is well known to practical opticians that it is more difficult to secure a satisfactory performance when reflection is oblique. The discrepancy is explained in great measure when we take into account the kind of error to which surfaces are most liable. No important deviation from a symmetrical form is to be feared; but a surface intended to be plane may easily assume a slight general convexity or concavity. Now in direct reflection a small curvature is readily and almost completely compensated by a small motion of the eyepiece giving a change of focus; but the compensation obtainable in this way is much less perfect when the reflection is oblique. In the first place the family of surfaces approximating to a plane, which will answer the purpose, coincides with the family of surfaces most likely to be produced; in the second case the family of ellipsoidal or hyperbolic surfaces capable (with suitable focus) of giving good definition contains only one symmetrical member—the perfect plane. In order to test experimentally the correctness of the theoretical result, it would be necessary to retain the focus suitable to the true surface, and not to allow a readjustment by which its errors may be in greater or less degree compensated.

A further difficulty, not touched by the preceeding considerations, still remains to be mentioned. In the ordinary method of testing plane surfaces by measuring the change of focus required when a distant point is viewed through a telescope, first directly,

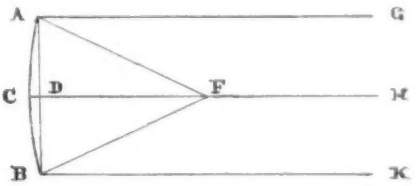


Fig. 8.

and then after reflection in the surface, the test is found to be more delicate as the reflection is more oblique. The explanation of the apparent inconsistency will be best understood by a calculation of the focal length of mirrors founded directly upon the principles of the wave theory. Let ACB (Fig. 8) be the arc of a (parabolic) mirror, which reflects parallel rays GA, HD, KB, to a focus F. $AD = y$, $DF = f$, $CD = t$.

In calculating the retardations of the various rays we will take as standard the phase at F of a ray coincident with HD, reflected at D, (as by a plane mirror ADB) instead of at C, so that the actual retardation at F of the central ray HCF is $2t$. The retardation of the extreme ray GAF is $AF - FD$, or

$$\sqrt{f^2 + y^2} - f$$

Since F is by supposition an optical focus the phases of all the rays must be the same; and thus

$$\sqrt{f^2 + y^2} - f = 2t \qquad (1)$$

If the aperture $(2y)$ be small in comparison to the focal length,

$$\sqrt{f^2 + y^2} - f = \frac{1}{2}\frac{f^2}{y} \text{ approximately,}$$

so that

$$f = \frac{y^2}{4t} \qquad (2)$$

In the limit it is a matter of indifference whether f be measured from D or from C. If r be the radius of curvature at C

$$t = \frac{1}{2}\frac{y^2}{r}$$

and

$$f = \frac{1}{2}r \qquad (3)$$

the usual formula.

When the incidence is oblique there are different foci in the primary and secondary planes. Considering first the case of the primary plane, let ACB (Fig. 9), be the mirror and F the focus, $AD = y$, $CD = t$, $HCD = \varphi$.

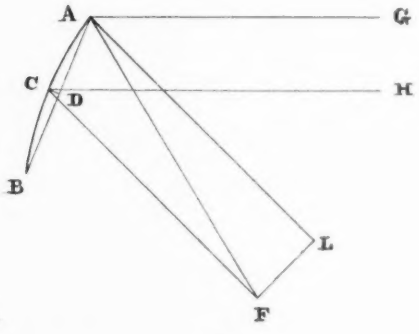


Fig. 9.

AL is the course which the ray GA would take if reflected by plane surface ADB. C is that point of the mirror at which the tangent is parallel to AB. The retardation at F of the ray GAF is $AF - AL$; and the retardation of the ray HCF due to the curvature of the mirror is $2t \cos \varphi$. These retardations must be equal; and thus

$$AF - AL = \sqrt{AL^2 + FL^2} - AL = 2t\cos\varphi$$

When the aperture is small $\sqrt{AL^2 + FL^2} - AL$ is approximately

$$\frac{1}{2}\frac{FL^2}{AL};$$

ultimately AL may be identified with f_2, the focal length in the primary plane, and FL may be identified with $y\cos\varphi$, so that

$$\frac{y^2\cos^2\varphi}{2f_2} = 2t\cos\varphi \qquad (4)$$

or

$$f_2 = \frac{y^2\cos\varphi}{4t} \qquad (5)$$

Thus it appears that, so far as the primary focal length is concerned, the diminished retardation of the central ray due to obliquity is outweighed by the corresponding diminution of effective aperture (FL); but although in consequence of obliquity a greater change of focus (estimated from that required for a plane surface) is necessary in order to get the best result, still if no change of focus be admitted, the error due to curvature is less sensible in oblique than in direct reflection.

The preceeding discussion assumes that the same extent of surface is used in all cases. In testing planes by reflection it often happens that a *greater* extent of surface is used in the case of obliquity, the field being limited by the object glass of the telescope rather than by the reflecting plane. Under such circumstances the loss of definition (with focus unaltered) due to curvature is aggravated by obliquity.

In the secondary focal plane there is no diminiation of effective aperture due to obliquity. Instead of (4) we have

$$\frac{y^2}{2f_2} = 2t\cos\varphi \qquad (6)$$

or

$$f_2 = \frac{y^2}{4t\cos\varphi}$$

In this case the favorable effect of obliquity shows itself directly in the increased value of f_2."

In conclusion it may be said that it is certainly time to discriminate between different classes of work, when discussing the relative merits of reflecting and refracting instruments. Neither the reflector nor the refractor can be considered as the more efficient *in all classes of work* and any argument which attempts to force such a conclusion necessarily loses in power by its very at-

tempt to prove too much. In the general field of visual observations and in astrophotographic work the refractor is for many reasons superior to the reflector, but in spectroscopic, spectrographic and allied branches of astrophysical investigations, the latter instrument is far more efficient than the former. This has been so often pointed out and so abundantly demonstrated by the results of observation that, considering the importance which spectrotcopic research has now assumed in the development of modern astronomy, it seems surprising that on the one hand the claims of the reflector should not be more generally recognized in the equipment of modern observatories, and that on the other some steps should not have already been taken to turn the large reflectors already constructed to account in the investigation of some of the many problems now pressing for solution in this already vast and daily extending field of research; the opening of which, little more than thirty years ago by the pioneering labors of Huggins, Vogel, Young and others, makers, it is safe to say, the beginning of an era which will be even more fruitful of astronomical discoveries and possibilities of development of astronomical knowledge, than that with which the name of the Herschels will remain forever associated.

YERKES OBSERVATORY.
24th Dec., 1897.

III. ON THE BEST METHOD OF OBSERVING THE MILKY WAY.

A. PANNEKOEK.

FOR POPULAR ASTRONOMY.

The aim of all our researches of the Milky Way is to get a picture of all its details representing as correctly as possible its appearance to the naked eye. It is only on account of its extraordinary faintness that we are obliged to use special methods and precautions in this research. Among these precautions the following are the most important.

The observer should not be acquainted with the results of former researches. For its great faintness makes it very easy to see, what we expect to see; and preconceived ideas will soon vitiate the results. Another important precaution to be taken is to avoid the inconvenience caused by artificial illumination. This diminishes the sensitiveness of the eye to the faint glow of the Milky Way and small differences of brightness remain unper-

ceived. It is often observed that sometimes a quarter of an hour after leaving a lighted room the eye is still dazzled. Therefore good observations of the Milky Way can only be made when the eye has been some time in darkness, and it is most earnestly advised to write in darkness without artificial illumination of the paper.

It stands to reason that the sky should be wholly clear and cloudless. Even the slightest veiling of the sky will wholly vitiate the aspect of the Milky Way, and observing should in this case be wholly avoided. Moonlight too may be very injurious; not only making the faintest parts of the Milky Way invisible, but also by the general illumination of the sky and of the surroundings causing the eye to become insensible to slight differences of brightness.

Some difficulty is presented in observing all these precautions, but in every case the observer should take care to note all the circumstances that may perhaps diminish the value of the observations, so that afterwards each of them may be valued according to its comparative certainty.

When an astronomer takes up this line of research he should first make himself acquainted with all the stars in the Milky Way regions. At the same time he may take a survey of the principal spots and streamers of the Milky Way itself, for only when the observer knows the general aspect, will he be able to study the further problems successfully. These may be divided into two different lines of research, viz, the investigation of the minutest visible details and the examination of the general distribution of brightness.

When we wish to follow the first line, it is best to study at once, only a limited part e. g along a longitude of 30 — 60 degrees. This part is surveyed till we know exactly what we see, and as soon as we can describe in words all its peculiarities we note them down. In this manner we describe the position of bright and dark spots and their boundaries by the stars on them; also the streamers, which connect these spots, or which lose themselves in the darkness of the background. Moreover we notice the appearance of a spot, whether it is equally luminous, or whether it diminishes from one side to another, whether its boundaries are marked or whether the light gradually fades into the surrounding darker parts; whether the light is even or whether it is dotted by numerous little stars, or clusters to little flakes of light. In bright spots it is often advantageous to look at dark lanes, that divide them, and I have always fonnd it best

to look not exactly at the point that is being examined but somewhat above or at one side of it. By this indirect vision minute details appear, that are not visible when looked at directly. If desirable the description can be completed by little sketches, e. g. of irregular boundary lines. The brightness of the different spots may be designated by numbers; these will never leave doubt as to what the observer has seen, and therefore are much clearer than the adjectives faint, bright, very bright, etc. A scale with 5 for the brightest spot, 0 for the background of the sky will, I think, suffice.

When the observer has in this manner filled up his sheets of paper with the description of a part of the Milky Way, they should be copied at home as soon as possible; for often the lines will be confusedly and illegibly written and he will then still be able to recollect what is meant. A sketch of the examined region can be added, not as an exact representation, but only to supplement the description and make it clearer.

The same region should be examined several times in this manner, and the observer will soon perceive that during subsequent observations he is able to see more peculiarities than at the first. In midsummer the regions of Aquila and Ophiuchus can be taken up, and during autumn the further course of the Milky Way through Cygnus, Cassiopeia and Perseus is examined, till in winter the research is concluded with the Auriga and Orion regions. The next year the observations are repeated, till a sufficient quantity of materials is collected for a good general description, which nowhere leaves doubt about the appearance of the Milky Way.

Particular emphasis has been laid on the verbal description; indeed it is much more intelligible to every one than a picture; for a picture gives the opportunity of doubting of what is seen by the observer, especially when the inaccuracies of the multiplying process add to the impossibility of drawing everything exactly as we desire it. On the other hand a picture has the advantage of showing in a moment what would require a long verbal description. Whoever therefore gives preference to drawing for this purpose can make himself very useful by drawing an accurate picture of the Milky Way. But I believe this will be much more difficult.

In taking up the second problem, the investigation of the general distribution of brightness, the observer should pay attention only to the principal peculiarities, that are perceived by an experienced observer at first sight; greater parts of the Milky Way may then be treated at once. The method, that has pleased me

most, is by drawing lines of equal brightness (isophotic lines). After having examined the region thoroughly, a boundary line is picked out and its course is followed along the Milky Way, everywhere tracing the places of equal brightness. After having finished such a line and after having marked its course upon the chart, another is chosen, shaping its course along a track of greater or lesser brightness. It will be necessary often to compare the brightness of more distant parts. It will soon appear that only 4 or 5 different degrees of brightness can be used without confusion. The different regions that are examined one after another must be linked together by parts they have in common; and comparison of regions at great distance will control their relative brightness. In this research the parts of the Milky Way examined should not be at too low an altitude for then the extinction of their light by our atmosphere has too great influence.

For these observations it is very useful to have charts, that can be used without artificial illumination. Such charts may be easily constructed by taking dull-black paper (which can be made e. g. by covering the paper with a coat of blacking and starch mixed) and dotting the stars upon it with a white body-color *f. i.* white-lead. When the Milky Way diagrams of Marth are divided by means of the strokes along the edges into squares of 5°, and when a similar net on a larger scale is drawn upon the paper, the squares can be filled up with stars by eye estimate. On such a chart chalk may be used to draw the lines; when they are copied at home on the white charts, the lines may be effaced by means of a wet sponge.

Another means of getting materials for the general distribution of brightness is to compare the brightness of spots along the whole Milky Way, at small and at great distances. The brightness may be expressed by numbers from 1 to 5; and in this manner a catalogue may be constructed where each spot or streamer has its brightness expressed in the same manner as a star by its magnitude. The place can at first be given by alignments of small stars in the neighborhood; and afterwards at home by their galactic longitude and latitude according to the diagrams of Marth. Perhaps it is useful to choose standards of brightness in different parts of the sky; these should be chosen, where greater spots of even light occur.

For the advancement of science it is necessary to publish the immediate results of both methods of working. The study of the general distribution of brightness may give great profit, in connection with star gauges, in researches on the construction of the

universe, like Herschel's, Struve's and Gould's, while the study of minute details may be useful in such researches as Easton's as to the connection of the distribution of the smallest stars with the appearance of the Milky Way, and in the future, to discuss the question, whether changes in the appearance of the Milky Way take place. But as it will be a great satisfaction for an observer to have a picture as exact as possible, I will explain the manner in which to construct it from the observations; moreover it may make the foregoing explanations somewhat clearer. When on a star chart we draw the lines of equal brightness, described on p. 525-6 by shading the region between them with increasing deepness, then blending these shades into one another at their boundary lines, we have a picture with good distribution of brightness, but showing only the more general details, though it gives the general appearance very well. Upon this background we can draw the minutest peculiarities, taken from the results of the studies of p. 526-7 we then have a picture that contains all that the observer has been able to see, and which still shows the brightness of the different parts in a very exact proportion.

ON THE "WORTHLESSNESS"* OF METHODS OF GEOMETRICAL OPTICS IN DEALING WITH THE PROBLEMS RELATING TO THE DEFINITION AND THE DELINIATING AND RESOLVING POWER OF TELESCOPES.

F. L. O. WADSWORTH.

FOR POPULAR ASTRONOMY

It has often happened in the history of science that the most celebrated men of their time have, through their conservatism or worse yet active opposition to new ideas, greatly retarded the progress of science. So it was in the case of Berthollet who in the

* I feel that an apology ought to be offered for using such a strong expression as this in the title of a paper. It is not mine but is borrowed from the title of a recent article in the *Astronomical Journal* ("On the 'Worthlessness' of a Proposed Form of Telescope,") by Professor Schæberle. If however the expression was justifiable in that case, it will be admitted, I think, that it certainly is in the present one; (see note "On a New Form of Mirror for a Reflecting Telescope," which appears elsewhere in this issue.)

In this article Professor Schæberle has remarked that "certain fundamental principles of optics have remained hidden ever since the invention of the telescope." It will hardly be admitted I think that physicists and opticians at least, are in general so ignorant of one of the most important branches of their subject as Professor Schæberle would have us believe by his statement, although it can not be denied that there are some astronomers who are either ignorant of or choose to ignore, "certain fundamental principles."

zenith of his reputation brought all of his influence to bear against the acceptance of the law of chemical-combination advanced by Proust, the final triumph of which marks one of the great forward steps in the history of chemistry; in the case of Cuvier, the great French biologist, who in common with most of his contemporaries, looked upon the doctrine of transmutation, advanced by Lamarck, the predecessor of Darwin by nearly half a century, as the idea of a dreamer; in the case of Davy and Wolf Iatson who if they did not actively oppose Dalton's atomic theory, the basis of our present conceptions of the constitution of matter, at least looked upon it with little favor; in the case of Faraday, Brewster and Herschel who adopted a similar attitude toward the doctrine of the Conservation of Energy, now justly regarded as one of the grandest conceptions of any age or century, when it was first brought forward by Joule and Mayer; finally in the case of Newton and his followers who by their opposition to the wave theory of light, delayed its general acceptance for more than a century.* It is not improbable that the influence of this great master of geometricians, greater perhaps at the present day among astronomers and mathematicians than among physicists, has had something to do with the pernicious persistence of geometrical methods of treatment in both optics and, at least in England, in pure mathematics. Considering however that it was one of the great astronomers of the present century, Airy, who was among the first to recognize the claims of the physical theory and to develop the methods of physical optics, and that the subject has since been so ably extended by Stokes, Cornu, Rayleigh, Lommel, Michelson, Struve, Andrè and many other of the most distinguished physicists and astronomers of the last half of this century; it is indeed surprising that the conclusions of this theory should be so indifferently recognized, and its methods of analysis so completely ignored by many astronomers of the present day.

I do not by any means wish to imply that the methods of geometrical optics are without value, or that the conclusions reached when they are employed are always erroneous. On the contrary I regard these methods of the greatest value, in computing the curves of lenses for visual or for the various classes of photographic work, in determining principal and conjugate foci of various lens combinations, in determining indices of refraction,

* It is perhaps worth noting that the wave theory was even more strongly opposed in France than in England, the home of Newton. Laplace, Poisson and Biot all waged bitter war against it. Such is the weakness of genius.

the deviations and dispersions of various prism trains etc., and in dealing with many allied problems in which we are concerned principally with a *change in direction* of rays (sic) of light. When however we come to deal with problems in which we are more directly concerned with questions relating to *a change in the form of the wave front*, such as questions relative to the phenomena of diffraction, interference and double refraction, the methods of geometrical optics are as much out of place and as little capable, when employed, of yielding results which are either complete or reliable, as purely geometrical methods of analysis would be in dealing with the general problem, (for example) of a body moving in a resisting medium of varying density, or under the action of varying forces.

In dealing with questions as to the form and character of telescopic images, in the formation of which the phenomena of diffraction play a fundamental part, conclusions have frequently been reached by writers, who have used the methods just alluded to, which are not only incomplete, but, frequently entirely wrong and misleading. The writer has already called attention to many cases of this kind* and it now seems necessary to refer to yet another one. In a recent paper published in the *Astronomical Journal*,† Professor Schaeberle has derived certain formulæ for what he terms the "blurring factor" of a parabolic mirror, a term the necessity for whose introduction, by the way, it is difficult to understand, since it is simply another expression for what

* See papers (1) "General Considerations Respecting the Design of Astronomical Spectroscope," *Astrophysical Journal*, Jan. 1895, pg 52; (2) "Further Notes on Astronomical Spectroscopes," *ibid*, March, 1896, pg. 176; (3) "Conditions of Maximum Efficiency in the Use of the Spectrograph," *ibid*, May, 1896, pg. 321; (4) "The Resolving power of Telescopes and Spectroscopes for Lines of Finite Width," Mem. d. Soc. d. Spet. Ital., Jan. 1897, pg 2, Phil. Mag., May 1897, pg 317. Wied Ann, June 1897, p 604, and Jour. de Phys. Aug., 1897 p 409; (5) "On the Conditions which Determine the Limiting Time of Exposure of Photographic Plates in Astronomical Photography," *Ast. Nachr.* No. 3439, and *Knowledge* Aug. and Sept., 1897 pps. 193–218; (6) "On the Conditions of Maximum Efficiency in Astrophotographic Work. Part I. General Theory of Telescopic Images." *Astrophysical Journal*, Aug. 1897. p 119; (7) "On the effect of the Size of an Objective on the Visibility of Linear Markings on the Planets," *Astronomical Journal*, No. 414, p 41; (8) "On the Photography of Planetary Surfaces," Observatory Sept. Oct. Nov. 1897, pp 333, 365, 404, etc., etc. In Nos. (5) and (6) there is an error in one of Lord Rayleigh's results which was accepted and used by the writer in developing the general theory of the "delineating power" of a telescope; but this error fortunately affects only one of the minor conclusions (that relating to the fogging of the photographic plate by the light from the sky) of these papers. See note on "An Error in the Expression for the Intensity of Illumination at the Focus of a Telescope," *Astronomical Journal*, No. 424, also *Astrophysical Journal*, Jan. 1898.

† "In a fundamental Optical Defect in the Image Formed by a Parabolic Reflector" A. J. No. 413 p 35; see also paper published in recent number of this Journal, "Theoretic Cause of the Failure of Parabolic Reflecting Telescopes of Large Angular Aperture," POPULAR ASTRONOMY, Oct. 1897, p. 315.

is ordinarily termed spherical aberration.* Applying these formulæ to the case of various existing instruments, the author comes to the conclusion "that a parabolic reflector having a ratio of focal length to aperture which is less than, say, fourteen to one, is theoretically and practically unfit for making observations of extreme delicacy" . . . . . "even then" (with this ratio) "it is essential that the center of the field of view shall coincide with the optical axis, and all observations made within a few seconds of arc from this axis."

In commenting upon these conclusions, which are at utter varience with results of observations to which I shall presently call attention it might be remarked first of all, that Professor Schæberle's formulæ, as Professor Poor has also recently pointed out,† are "only approximate and are derived in an unsatisfactory and obscure manner." It is not to this however that the errors in Professor Schæberle's conclusions can be ascribed for his formulæ, approximate though they are, give results fairly accordant with these yielded by the formulæ derived in a more rigorous manner by Professor Poor. Professor Schæberle in common with the majority of the preceding writers on this subject has been led astray in his conclusions by forgetting that the question of the linear extent or "spreading" of an image in the focal field of a telescope is in most cases entirely subordinate in importance to the question of the *distribution of light* in that image. As a good illustration of this important point it is only necessary to consider the character of every physical image, either of a star or of an extended source, (not infinite in extent) which is formed by a telescope. Even when the instrument is entirely free from both chromatic and spherical aberation, and the 'seeing' is perfect, the image is 'spread' (by the effect of the diffraction of the finite aperture of the image forming lens or mirror), over the whole of the focal plane. This 'spreading' however produces but little injurious effect on the definition because a large proportion of the light is concentrated within the borders of the geometrical image (in the case of an extended source,) or within the central element of the diffraction pattern, (the so called spurious disc), in the

* In passing it may be remarked that the assumption made in Professor Schaeberle's paper that this question (spherical aberration of mirrors), had previously entirely escaped the attention of astronomers and opticians is certainly a surprising one. It has been considered by a number of writers among whom Lieut. Gen. Tennant deserves special mention. His paper on this subject, which will be referred to again later on, was published ten years ago in the Mon. Not. R. A. S., (Vol. 47 p 244, March, 1887) and the treatment of the special cases he considered is more complete and satisfactory than that of any of the other writers on this subject, with whose papers I am acquainted.

† See paper "On the Aberration of Parabolic Mirrors." A. J. No 420, p. 89.

case of a star. Nevertheless we should never loose sight of the fact that no physical image has a definite edge or boundary, the apparent edge being produced by the contrast effect, resulting from the rapid falling off in the intensity of the diffraction pattern at that point.

A still more striking illustration of the primary importance of the question of the distribution of light in the image as compared with the question of the linear extent of the latter, is afforded by the consideration of the images produced by an ordinary refractor.

In this case the images are broadened not only by diffraction but to a much more serious extent by chromatic aberration. The color curve of the 40-in. telescope has recently been determined by the writer* and it has been found that the difference in focus between yellowish green light, of wave length about 5500 Angstöm units, (which is about the mean wave length for which the telescope has been corrected, or as it is sometimes termed the mean wave length for minimum focus), and blue light of wave length about 4500 Angström units, is nearly 1.6 inches. When the telescope is focussed for light of the first wave length (which for most observers gives the sharpest visual image), the blue image of a star disc, (for example), will be out of focus by the above amount, and its diameter, considering only the central element, the so called spurious disc, of the image, will be about 0.12 in. or over 20″. On such grounds of comparison then (since this "blurring" would occur even at the very center of the field), the refractor would be very much worse than the reflector, would in fact be "utterly worthless." But we know of course that under proper atmospheric conditions the definition of a good refractor is at least sufficient to enable us to very nearly if not quite realize the theoretical resolving power of its aperture, which is certainly all that we could expect of any instrument. The reason for this again is found in the fact that notwithstanding this great broadening of the image, (which as just pointed out, would seem on the ground of geometrical optics, to be prohibitive of any good results being obtained), the actual distribution in intensity in what I have elsewhere termed, "the visually effective diffraction image"† is such that it is the central part only which affects the eye to any degree.

In the case of the parabolic mirror by far the larger proportion of the light which unites to form an image of a

* A full report of this investigation will be published in the Feb. or March number of the *Astrophysical Journal.*

† "General Theory of Telescopic Images," *Astrophysical Journal.* Aug. 1897.

point source, comes from the outer portion of the mirror, particularly when the instrument is of the usual Newtonian or Cassegranian form. To attach equal importance to the effect of the inner and outer zones in determining the prejudicial effect of aberration is therefore fundamentally wrong.

To those who prefer to draw conclusions from the results of practical observation, rather than from the preceding considerations, which they might perhaps consider of a more or less theoretical nature, (and there are some astronomers, to whom, for some reason best known to themselves, theoretical considerations, seem to appeal very slightly if at all), the following facts, which it is certainly surprising Professor Schaeberle should have so completely forgotten or overlooked, will prove even more convincing against the conclusions deduced from a geometrical treatment of the problem under consideration. Herschel discovered the two inner satellites of Saturn with his celebrated 40-ft. reflector *used in the Herschelian or "front view" form*. The ratio of aperture to focal length in this instrument was 1 to 10 and the images must therefore have been observed at least 1°. 5 from the optical axis. At this distance the image of a point source would, according to the results of Schæberle have a diameter, (maximum), of at least 10″ *which is greater than the distance of the inner satellite* (Mimas), *from the edge of the ring at the time of discovery*. Even still better evidence of the incorrectness of the conclusions under discussion is found in Herschel's measurements of double stars made with the same instrument between 1802 and 1804. Among these may be mentioned, (I include in the list only those whose separation at that time was very much less than 10″,) α Leonis, (less than 1″); ζ Ursæ Majoris, (less than 3″); η Coronæ Bor. (about 1″.5); σ Coronæ Bor., (about 1″.5); μ^2 Boötis, (less than 2″), 4 Aquarii, (about 0″.7); τ Ophiuchi, (*less than 0″.3*); and many others equally close. Later Sir John Herschel used his 20-ft. front view reflector with equal success in measuring the close double systems in the southern hemisphere. [γ Centauri, (about 0″.6); γ Coronæ Australis, (3″.0), etc., etc.] Further evidence on this point is found in the recent photographs of Roberts, Wilson, and Common, taken with reflectors of still larger angular aperture *i.e.* 1 to 5. I have not had an opportunity of examining any of the original negatives taken with these instruments but I have measured the diameter of a number of the star images on the published reproductions of Robert's photographs,* and find many of them at the

* Celestial Photography; by Isaac Roberts, London, 1893.

extreme edge of the field (more than 1° from the center) certainly do not exceed 5″. According to Schæberle's and Poor's results "the disc of a star 1° from the center of the field would exceed half a minute in diameter."*

The results obtained by the writer with various forms of spectroscopes in which mirrors were used in place of the usual lenses, in the collimator and view (camera) telescopes also offer emphatic evidence of the high efficiency of mirrors even when the field of view, (in this case the spectrum), is considerably extended and the part used quite a distance to one side of the optical axis.†

It will be evident I think without further discussion that the conclusions to which writers have been led by employing the methods of geometrical optics, are as incomplete, (and therefore untrustworthy), in this case as they have been found to be in others.

Gen. Tennant, in the paper already referred to, partially investigated the distribution in intensity in the image of a point near the center of the field but even his results for this one case, are incomplete, as he himself recognizes, on account of his neglecting to take into account the effect of diffraction. In a recent letter to me he expressed the hope that I would take up this investigation, but the pressure of other work in which I am now engaged has prohibited my undertaking it.‡ The complete expression for the distribution in intensity in the monochromatic image of a point at any part of the focal field, is

$$I^2 = \frac{1}{\lambda^2}\left[\int\int \frac{1}{\rho} \cdot \sin \frac{2\pi}{\lambda}\,(\alpha t - \rho - \varepsilon)\,dx\,dy\right]^2 \qquad (1)$$

where λ is the wave length of light, ρ the distance of any point, p, in the focal plane (at which the intensity is I^2) from the element $dx\,dy$ of the surface of the mirror, and ε the distance of this ele-

* This would be its maximum diameter; the minimum diameter (at right angles to this), would be slightly less.

† See papers. Report of the Smithsonian Astrophysical Observatory for 1893, ditto for 1894. Smithsonian Reports, Washington: "An Improved Form of Littrow Spectroscope." Phil. Mag. July 1894, pg 137; "Fixed Arm Spectroscopes," ibid Oct. 1894, pg 337: "The Modern Spectroscope" IX, Astron. and Astrophysics, Dec 1894, pg 835; etc.

‡ The following extract from Gen. Tennant's letter, which I have taken the liberty of quoting, shows that this investigator had a clear appreciation of the proper method of considering the problem. He says, "My paper to which you refer was written in a great measure with reference to inquiries by Dr. Common as to the examination of his large mirror which was then in hand: for this purpose the introduction of the physical theory would have been out of place: of the necessity of considering that I was convinced if the question were to go farther, and it was this conviction which withheld me from making any attempt to estimate the distribution of light in the image of a star out of the center of the field. But I think that it would be a valuable investigation, etc."

ment from the incident wave surface in which the vibration is represented by $\cos \frac{2\pi}{\lambda} at$. The integration is extended over the whole of the surface of the mirror.

Let us call $\xi\eta$ the coördinates of the point p (referred to the center of the field) and x, y, z the coördinates of the element $dx\,dy$, with reference to the same origin. Then if f denote the principal focal length of the mirror and m the parameter of the parabolic surface, we will evidently have for ρ

$$\rho = \sqrt{(x-\xi)^2 + (y-\eta)^2 + z^2}$$

$$= \sqrt{(x-\xi)^2 + (y-\eta)^2 + \left(f - \frac{x^2+y^2}{m}\right)^2} \qquad (2)$$

and for ε

$$\varepsilon = x\cos\alpha + y\cos\beta + \left(f - \frac{x^2+y^2}{m}\right)\cos\gamma - l \quad (3)$$

where α, β, γ are the direction cosines of the incident wave front (supposed plane), and l the distance of the chosen wave front, (in which the vibration is represented by $\cos \frac{2\pi}{\lambda} at$), from the central focal point.

When ξ, η, are small and f is large the expression for I^2 is readily reduced to the form

$$I_1^2 = \frac{1}{\lambda^2 f^2}\left[\int\int \sin\left(\frac{2\pi\xi}{\lambda f}x + \frac{2\pi\eta}{\lambda f}y\right)dx\,dy\right]^2$$

$$+ \frac{1}{\lambda^2 f^2}\left[\int\int \cos\left(\frac{2\pi\xi}{\lambda f}x + \frac{2\pi\eta}{\lambda f}y\right)dx\,dy\right]^2$$

which has already been investigated.*

The investigation of the general integral, (1), in the case of a parabolic mirror is not in the opinion of the writer, of much *practical* importance, for the reason that the most useful field of work for reflectors is stellar spectroscopic and allied branches of astrophysical work, in which only the very center of the field is utilized†. Such an investigation however would have great theoretical interest and it is to be hoped that Professor Schæ-

* See Rayleigh's Wave Theory, Enc. Brit. Vol. 24, §§ 11 and 12, also paper already referred to "General theory of Telescopic Images," *Astrophysical Journal*, Aug. 1897.

† For further remarks on this point see my previous papers in the Astrophysical Journal, Jan. 1895, p 52, March, 1895 p 232, March, 1896, p 182-3, Feb. 1897, p 132 etc.

berle or Professor Poor* who have begun it on the lines of geometrical optics will complete it by the methods of physical optics and thus obtain for the first time complete results which will enable us to judge of the character as well as the mere size, of the images formed by a reflector at some distance from the optical axis.

Owing to the concentration of the attention of opticians during recene years on the problem of the improvement of the refractor the subject of the reflector has not received the general attention that it should from either the theoretical or the practical side.†

If one-tenth the time had been devoted to the problems arising in connection with the figuring, mounting and use of specula as has been spent on the computation and figuring of the lenses of refractors, the reflecting telescope would occupy a much higher place in the estimation of astronomers and spectroscopists, than it does at present. As an example of the really primatic state of our knowledge of the theory of the reflector, it need only be remarked that it is not at all certain that the parabolic form of surface is the best for mirrors, a fact which was long ago pointed out by Lord Rayleigh.‡

YERKES OBSERVATORY,
Dec. 8, 1897.

TRANSITS OF VENUS AND MERCURY.

J. MORRISON, M. A., M. D., PH. D.

FOR POPULAR ASTRONOMY.

A transit of Venus or Mercury may be regarded as an annular eclipse of the Sun, the planet taking the place of the Moon, but with this difference viz: When the Moon is the eclipsing body the umbral penumbra covers only a small portion of the Earth's

* I have just received word from Professor Poor that he proposes to take up this investigation. See his paper, "Aberration of Parabolic Reflectors" which will be published in the Feb. number of the *Astrophysical Journal.*

† In this connection it is only proper to refer to Sir Howard Grubb's loyal and constant advocacy of the reflector for many years past. See particularly his papers "Great Telescopes of the Future," Trans R. Dub. Soc. New Ser. Vol. I Mem. 1; "On the Choice of Instruments for Stellar Photography," Mon. Not., Vol. 47 p. 301, April, 1887; "The Development of the Astronomical Telescope," Roy. Inst., May 25th, 1894 and many others. As has been indicated before the writer does not agree entirely with Sir Howard as regards the problems of astronomical photography, but with respect to astrophysical work he is in entire accord with him.

‡ "Investigations in Optics with special reference to the Spectroscope." § 1. "Resolving or Separating Power of Optical Instruments," *Phil. Mag.* Oct. 1879, p. 261.

surface, but in the case of the planet it envelops the whole Earth.

As seen from the center of the Earth, a transit will begin and end when the planet is tangent to the cone which envelopes the Sun and has its vertex at the Earth's centre. For the Earth generally it begins and ends when the planet is tangent to the cone which envelopes the Sun and Earth.

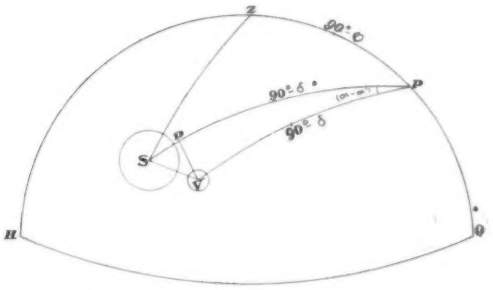
The formulæ for solar eclipses are applicable to transits but in (63) we must use for Venus $k = 0.9975$ and for Mercury $k = 0.3897$, and the rigorous formulæ (49) (50) and (51) must be used for the computation of a, d and g.

As the parallaxes of both planets are very small, it will however be found more convenient to employ the special method of Lagrange as herein modified and improved.

To Find the Times of Ingress and Egress as seen from the Centre of the Earth.

The elements are computed as in the case of eclipses and the parallaxes and semi-diameters of the bodies may beregarded as constant during the period of transit.

Let S represent the centre of the Sun and V that of the planet as seen from the centre of the Earth, and P the north pole, then in the spherical triangle PSV we have $PS = 90° - \delta'$, $PV = 90° - \delta$ and $SPV = \alpha - \alpha'$.



Put $SV = \Delta$ and $PSV = Q$, then we have

$$\begin{aligned} \sin \Delta \sin Q &= \cos \delta \sin (\alpha - \alpha') \\ \sin \Delta \cos Q &= \sin \delta \cos \delta' - \cos \delta \sin \delta' \cos (\alpha - \alpha') \\ &= \sin (\delta - \delta') + 2 \sin \delta' \cos \delta \sin^2 \frac{1}{2} (\alpha - \alpha') \end{aligned} \qquad (184)$$

or since Δ, $\delta - \delta'$ and $(\alpha - \alpha')$ are quite small we may substitute the arc for the sine and express them in seconds thus

$$\Delta'' \sin Q = (\alpha - \alpha')'' \cos \delta$$

$$\Delta'' \cos Q = (\delta - \delta')'' + \frac{2 \sin \delta' \cos \delta \sin^2 \frac{1}{2} (\alpha - \alpha')}{\sin 1''} \qquad (185)$$

$$= (\delta - \delta')'' + \beta'' \quad \text{suppose.}$$

Taking S as the origin of a system of rectangular coördinates and drawing VD perpendicular to SP we shall evidently have

$$\begin{aligned} \Delta \sin Q &= x = (\alpha - \alpha') \cos \delta = VD \\ \Delta \sin Q &= y = (\delta - \delta') + \beta \ = SD \end{aligned} \qquad (186)$$

Now if x and y be computed for several consecutive hours preceding and following conjunction so as to include the entire duration of the transit, their differences will furnish the hourly variations, x' and y', as in the case of solar eclipses.

Let x_0 and y_0 be the values of x and y at an epoch T_0 near conjunction, s' and s the semidiameters of the Sun and planet respectively and let the required time of contact for the centre of the Earth be $T = T_0 + \tau$, for which date $\Delta = s' \pm s$, the upper sign for external and the lower for internal contact, then we shall have

$$\begin{aligned} (s' \pm s) \sin Q &= x_0 + x'\tau \\ (s' \pm s) \cos Q &= y_0 + y'\tau \end{aligned}$$

Put $\quad m \sin M = x_0, \qquad n \sin N = x'$

$$m \cos M = y_0, \qquad n \cos N = y' \qquad (187)$$

then

$$\begin{aligned} (s' \pm s) \sin Q &= m \sin M + n \sin N . \tau \\ (s' \pm s) \cos Q &= m \cos M + n \cos N . \tau \end{aligned}$$

which solved in the usual way give

$$\sin \psi = \frac{m \sin (M - N)}{s' \pm s}$$

$$\tau = \mp \frac{(s' \pm s)}{n} \cos \psi - \frac{m}{n} \cos (M - N) \qquad (188)$$

$$Q = N + \psi$$

and $\quad T = T_0 + \tau$

To Find the Times of Ingress and Egress for a given Place on the Earth's Surface.

For the date T just found compute from the ephemeris the R.A. and Decl. of the Sun and planet and the values of x and y by (186), then the first of (187) will become

$$\begin{aligned} m \sin M &= x \\ n \cos M &= y \end{aligned} \qquad (189)$$

whence $\quad m^2 = x^2 + y^2 = (s' \pm s)^2$

or $\quad m = (s' \pm s)$

Let T' denote the time of ingress or egress for a given place whose zenith is Z, then $T' - T$ is the retardation or acceleration due to parallax and since T' can differ from T by only a few minutes, we may regard m as varying uniformly.

At the time T, the *geocentric* distance $= m = s' \pm s$ and at the time T' the *apparent* distance is also $= s' \pm s$, but at this latter date the geocentric distance has become

$$m' = m + (T' - T)\frac{dm}{dt} \qquad (190)$$

where $\frac{dm}{dt}$ = the change of m in a unit of time.

Differentiating (189) we have

$$\frac{dm}{dt}\sin M + m\cos M\frac{dM}{dt} = \frac{dx}{dt} = x' = n\sin N$$

$$\frac{dm}{dt}\cos M - m\sin M\frac{dM}{dt} = \frac{dy}{dt} = y' = n\cos N \quad \text{by (187)}$$

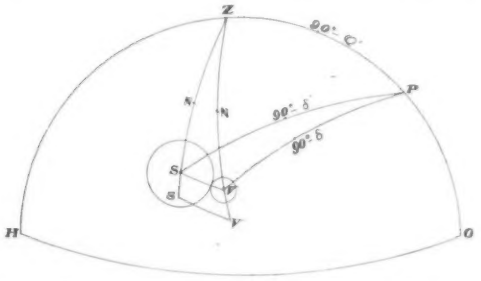
whence

$$\frac{dm}{dt} = n\cos(M - N)$$

which substituted in (190) gives

$$m' - m = (T - T)\,n\cos(M - N) = (T' - T)\,n\cos\psi \qquad (191)$$

because at the date T, $M - N = \psi$ by the first of (188)



If at the time of *geocentric* contact T, the observer were instantly transferred to a point on the surface whose zenith is Z, the bodies would be depressed by parallax to S' and V' respectively and the distance $SV = m = s' \pm s$ would become $S'V' = m'$ and we must now express $m' - m$ in terms of known quantities. Let the true zenith distances of the Sun and planet be denoted by z' and z respectively then $SS' = \rho\pi' \sin z'$ and $VV' = \rho\pi \sin z$ approximately and the spherical triangle ZSV gives

$$\cos VS = \cos ZV\cos ZS + \sin ZV\sin ZS\cos Z$$

or

$$\cos m = \cos z\cos z' + \sin z\sin z'\cos[Z$$

Differentiating, the angle Z being constant, we get

$$\begin{aligned} \sin m dm &= (\cos z \sin z' - \sin z \cos z' \cos Z)\, dz' \\ &\quad + (\sin z \cos z' - \cos z \sin z' \cos Z)\, dz \\ &= \sin m \cos S\, dz' + \sin m \cos V\, dz \end{aligned}$$

or $$dm = \cos S\, dz' + \cos V\, dz$$

Now for the differentials dm, dz' and dz we may without appreciable error, substitute the small increments $m' - m$, $\rho\pi' \sin z'$ and $\rho\pi \sin z$ respectively, therefore the last equation becomes

$$m' - m = \rho\pi' \sin z' \cos S + \rho\pi \sin z \cos V \qquad (192)$$

From the triangle ZSV we have

$$\begin{aligned} \cos z &= \cos z' \cos m + \sin m \sin z' \cos S \\ \cos z' &= \cos z \cos m + \sin m \sin z \cos V \end{aligned} \qquad (193)$$

Eliminating $\sin z' \cos S$ and $\sin z \cos V$ between (192) and (193) we have

$$\begin{aligned} m' - m = \frac{\rho}{\sin m} (&\pi' \cos z - \pi' \cos z' \cos m \\ &+ \pi \cos z' - \pi \cos z \cos m) \end{aligned} \qquad (194)$$

Now if h' and h denote the hour angles ZPS and ZPV of the Sun and planet respectively we shall have from the triangles ZPS and ZPV

$$\begin{aligned} \cos z &= \sin \varphi' \sin \delta + \cos \varphi' \cos \delta \cos h \\ \cos z' &= \sin \varphi' \sin \delta' + \cos \varphi' \cos \delta' \cos h' \end{aligned}$$

and substituting these values of $\cos z$ and $\cos z'$ in (194) we find after putting $\cos m = 1$ and writing m for $\sin m$ which can be done without appreciable error,

$$\begin{aligned} m' - m = \frac{\pi - \pi'}{m} \Big\{ &\rho \sin \varphi' (\sin \delta' - \sin \delta) \\ &+ \rho \cos \varphi (\cos \delta' \cos h' - \cos \delta \cos h) \Big\} \end{aligned} \qquad (195)$$

Let μ be the sidereal time at the first meridian (Greenwich) for the date T, then the hour angles of the Sun and planet at that meridian will be $\mu - \alpha'$ and $\mu - \alpha$ respectively, and the hour angles for any other place whose longitude is λ will be $\mu - \alpha' - \lambda = h'$ and $\mu - \alpha - \lambda = h$ but h' and h are very nearly equal, their difference being $\alpha - \alpha'$ which is, in this case, always quite small, we may therefore use without sensible error the mean value viz:

$$\frac{1}{2}(h' + h) = (\mu - \frac{\alpha' + \alpha}{2} - \lambda) = \ominus - \lambda, \text{ that is for } \cos h'$$

and $\cos h$ we may write $\cos (\ominus - \lambda)$ in (195) and we have

$$m' - m = \frac{\pi - \pi'}{m} \left\{ \rho \sin \varphi' (\sin \delta' - \sin \delta) + \rho \cos \varphi' (\cos \delta' - \cos \delta) \cos (\ominus - \lambda) \right\}$$

Substituting this value of $m' - n$ in (191) and solving for T' we have after some slight reductions

$$T' = T + \frac{2\,\omega\,(\pi - \pi') \sin \frac{1}{2}(\delta' - \delta)}{(s' \pm s)\, n \cos \psi} \left\{ \rho \sin \varphi' \cos \frac{1}{2}(\delta' + \delta) - \rho \cos \varphi' \sin \frac{1}{2}(\delta' + \delta) \cos (\ominus - \lambda) \right\} \quad (196)$$

where $\omega = 3600$, to reduce the terms to seconds and

$$\ominus = \mu - \frac{1}{2}(\alpha' + \alpha)$$

The angle Q found from (188) is the position angle or the angular distance of the point of contact measured on the Sun's limb from the north point towards the east, and is nearly constant for all places on the Earth.

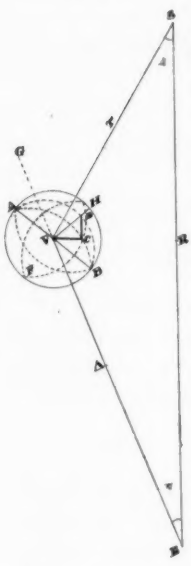
Disks of Mercury and Venus. *

The quantities referring to the disk and tabulated for every fifth day in the American Ephemeries, are defined on the same page. The formulae for their computation are easily deduced as follows:

* It was my intention to omit the formulæ for the disks of Venus and Mercury but when I recollected that they proved too difficult for a Professor of Mathematics in the U. S. Navy, I decided to develop them although extremely simple. Some seven years ago Professor W. W. Hendrickson who had been for over 17 years Professor of Mathematics at the Naval Academy, Annapolis, and the head of the mathematical department at that, was assigned to the Nautical Almanac office. He was *de facto* Superintendent of the office, acting as such in the absence of the actual Superintendent and succeeded for a while that official on his retirement last March. Professor H. being desirous of checking these quantities asked me for the formulæ which I did not then have already prepared, but I told him they were very easily deduced and that I always derived them *de novo* whenever I wanted them. This answer gave offense although none was intended. Failing in his attempts to deduce them with all the data in the office at his disposal, he was obliged to write to the regular computer, Mr. Austin of Salt Lake City, for a copy of them. At the investigation of the Nautical Almanac office held in July, 1893, he admitted under oath among other things that he was unable to deduce the formulæ, was not an astronomer, knew little or nothing about astronomy, never wrote a paper or made any observations or researches in the science and that his only astronomical achievement up to that time, was the calculation of the Sun's R. A. and Decl. from having given the obliquity and longitude, but even then he did not know how to take care of the Sun's latitude. This testimony, with much more of the same tenor, is on file in the Navy Dep't. It was the most astounding admission of ignorance and incompetency on the part of a Professor of Mathematics that ever came under my notice. He has recently been ordered back to Annapolis to assist the cadets in crossing the "pons asinorum" and other similar feats. I. M.

To Find k, the Ratio of the Illuminated to the Entire Disk.

Let S, E and V be the centres of the Sun, Earth and planet respectively, then VS is perpendicular to the plane of the circle $ACBF$ which separates the illumined from the unillumined portion of the planet, and EV is perpendicular to the plane of AHB, therefore the angle CVH = the angle GVS = $E + S$.



The semi-circle ACB when projected on the plane AHB will be the semi-ellipse ADB and $AHBDA$ is the illuminated part of the disk as seen from the Earth.

Let a = the semi-diameter of the planet seen from E then $a \cos (S + E)$ = the semi-axis minor of the ellipse.

$\frac{1}{2} a^2\pi \cos (S+E)$ = area of semi-ellipse ADB,

$\frac{1}{2} a^2\pi$ = area of the semi-circle AHB

$\frac{1}{2} a^2\pi \left(1-\cos (S+E)\right)$ = area of the lune or crescent $AHBDA$

and $a^2\pi$ = area of the whole disk

therefore
$$k = \frac{\frac{1}{2} a^2\pi \left(1 - \cos (S+E)\right)}{a^2\pi}$$
$$= \frac{1}{2}\left(1 - \cos (S+E)\right) \qquad (197)$$
$$= \frac{1}{2}(1 + \cos i)$$
$$= \cos^2 \frac{1}{2} i, \text{ where } i = \text{angle } EVS$$

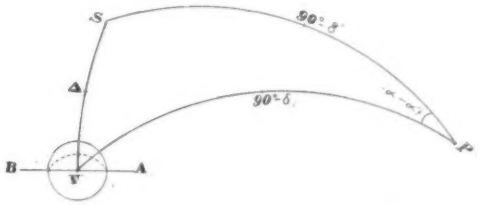
To Find i, which may be called the Parallactic Angle.

The Ephemeris gives for every Greenwich mean noon the logarithms of R, r and Δ, the three sides of the plane triangle EVS and with these data we have

$$\cos \frac{1}{2} i = \sqrt{\frac{s(s-R)}{\Delta r}} \text{ where } s = \frac{1}{2}(R + r + \Delta) \qquad (198)$$

TO FIND θ, THE ANGLE WHICH THE LINE JOINING THE CUSPS MAKES WITH THE MERIDIAN.

Let P be the north pole, S, the Sun, V, the planet and AVB the line joining the cusps, then $\theta = AVP$. The angle $SPV = \alpha - \alpha'$, the difference of right ascension, $PS = 90^\circ - \delta'$, $PV = 90^\circ - \delta$ and put $VS = \Delta$ then we have



$$\sin \Delta \cos V = \sin \delta' \cos \delta - \cos \delta' \sin \delta \cos (\alpha - \alpha')$$
$$\sin \Delta \sin V = \cos \delta' \sin (\alpha - \alpha')$$

Put $h \sin H = \cos \delta' \cos (\alpha - \alpha')$

$h \cos H = \sin \delta'$ that is $\tan H = \dfrac{\cos (\alpha - \alpha')}{\tan \delta'}$

then the above become

$$\sin \Delta \cos V = h \cos (\delta + H)$$
$$\sin \Delta \sin V = \cos \delta' \sin (\alpha - \alpha') \qquad (199)$$

whence $$\tan V = \frac{\sin H \tan (\alpha - \alpha')}{\cos (\delta + H)} \qquad (200)$$

and $$\theta = 90^\circ - V \ (+ 360^\circ \text{ if necessary}) \qquad (201)$$

θ being reckoned from 0° to 360°.

The quadrant in which V must be taken is determined by (199), h and $\sin \Delta$ being of course always positive.

TO FIND L.

Let a = the geocentric semidiameter in seconds of arc
d = the geocentric distance of the planet
then $\pi d^2 \sin^2 a$ = the area of the disk at distance d
$\pi k d^2 \sin^2 a$ = the area of the visible disk or crescent at same distance
and $\pi \sin^2 1''$ = the area of a disk of radius $1''$ at distance 1
= the unit of light

Now since the intensity of light varies inversely as the square of distance we have

Units of light received on crescent at distance $r = \dfrac{\pi k d^2 \sin^2 a}{\pi r^2 \sin^2 1''}$

$$= \frac{k d^2 \sin^2 a}{r^2 \sin^2 1''}$$

and therefore units of light received at distance $d = \frac{k \sin^2 a}{r^2 \sin^2 1''}$

or $$L = \frac{k a''^2}{r^2} \quad (202)$$

where a and r are given in the ephemeris.

ON NEWTON'S LAW OF GRAVITATION.

PROFESSOR H. SEELIGER.

FOR POPULAR ASTRONOMY.

It is also permissible to bring forward a second example for the case of a constant δ. We have, by (2);

$$X_2 = \delta \int_0^{2\pi} d\varphi \int_0^{\pi} P^1 (\cos \gamma) \sin \gamma \, (R_1 - R_0) d\gamma.$$

$$Z_2 = 2\delta \int_0^{2\pi} d\varphi \int_0^{\pi} P^2 (\cos \gamma) \sin \gamma \log \left(\frac{R_1}{R_0}\right) d\gamma.$$

If we assume here, for the law of increase of R, that m is a magnitude passing gradually to infinity and α a number greater than ½ and further that,

$$\log \frac{R_1}{R_0} = \alpha m + m P^2 (\cos \gamma),$$

We shall have;

$$X_2 = 2\pi\delta \, R_0 \, e^{\alpha m} \cdot \int_0^{\pi} \sin \gamma \cos \gamma \cdot e^{m\left(\frac{3}{2}\cos^2 \gamma - 1\right)} \cdot d\gamma = 0$$

and $$Z_2 = \frac{8\pi\delta}{5} m.$$

That is, X_2 remains equal to zero and the stress becomes infinite with m.

Other examples may without difficulty, be selected, in which δ is a different function of the place of the point. There can therefore, be no doubt, that the disclosed contradictions must remain for every possible and conceivable distribution of matter, if only δ possess the property emphasized, of having within infinitely great regions of space, finite values different from zero.

Light may of course, be thrown upon the unavoidable contradictions from various directions. They admit also of representation in another form. This will not be done here however, but we shall now draw an inference from the fundamental principles of the theory of the potential.

Throughout all regions of space the potential V, fulfills the condition:

$$\Delta V = \frac{d^2V}{dx^2} + \frac{d^2V}{dy^2} + \frac{d^2V}{dz^2} = -4\pi\delta. \qquad (3)^*$$

In conformity with the customary mode of thinking, let us imagine the universe filled with isolated, non-homogeneous bodies $m_1, m_2, m_3, \ldots m_n$, etc., with densities $\delta_1, \delta_2, \ldots$ etc., while at the same time the δ's are to fulfill certain conditions of continuity and moreover, the surfaces of the bodies must not possess certain singularities mentioned in the theory of the potential, since (3) and also Green's theorem are to be applied. We have then $\Delta V = -4\pi\delta$, within the mass m_1, and without the masses $\Delta V = 0$.

If the elements of volume be designated by $d\tau_1, d\tau_2, \ldots$, the superficial elements by $ds_1, ds_2, \ldots$ the normal to the surfaces of the bodies $m_1, m_2, m_3, \ldots$ directed inward by $n_1, n_2, n_3 \ldots$ Green's theorem gives,

$$\int \Delta V \, . \, d\tau_1 = -\int \frac{dV}{dn_1} \, . \, ds_1 = -4\pi \, . \int \delta d\tau_1 = -4\pi m_1$$

$$\int \Delta V \, . \, d\tau_2 = -\int \frac{dV}{dn_2} \, ds_2 = -4\pi \int \delta d\tau_2 = -4\pi m_2.$$

. . . . . .

If we consider an arbitrary, though closed surface F, including the masses $m_1, m_2, \ldots m_n$, we have,

$$4\pi (m_1 + m_2 + \ldots + m_n) = \int \frac{dV}{dn_1} ds_1 + \ldots + \int \frac{dV}{dn_n} ds_n \, .$$

If now, Green's theorem be applied to the space enclosed by F, and the surfaces of the masses $m_1, m_2, \ldots m_n$, within which $V = 0$, the last equation may be written thus:

$$\int \frac{dV}{dn} ds = 4\pi (m_1 + m_2 + \ldots + m_n).$$

Designating the arithmetical mean of all the $\frac{dV}{dn}$'s over the surface S, of F by $M\left(\frac{dV}{dn}\right)$, where also $S = \int ds$, we obtain

$$M\left(\frac{dV}{dn}\right) = \frac{4\pi (m_1 + \ldots + m_n)}{S}.$$

* In reading equation (3) and those following consider dV and d^2V, with denominators as expressions for partial differentiation. We have been unable to secure the usual type in time for use in this article.

If we imagine all the matter in the space R, which F comprises, whose volume is denoted by R, uniformly distributed, so as to have the uniform density δ_0; where

$$R\delta_0 = m_1 + \ldots + m_n,$$

we may then write the last equation thus:

$$M\left(\frac{dV}{dn}\right) = 4\pi\delta_0 \cdot \frac{R}{S}.$$

By increasing R, the right side of this equation may be made as great as we please. There must then, be indefinitely large values among the individual values of $\frac{dV}{dn}$. According to the theory of the potential, consequently, *infinitely great accelerations must occur in the universe*, and this would be true with *any conceivable mode of mass-distribution whatever*. There would therefore, result motions which, starting from finite velocities, would lead within a finite time, to infinite velocities. This conclusion contains within itself either an absurdity, or a direct contravention of the theory of mechanics.

It seems that such doubts concerning the absolute correctness of the Newtonian law as those adduced here are always received with suspicion and distrust, even though it is impossible to produce anything demonstrable against them. Probably the reason for this is to be attributed in part, at least, to the difficulty with which people make clear to their minds that Newton's formula, despite its unbounded success in explaining astronomical facts, is and can be nothing more than an empirical law. That it corresponds with an extremely high degree of approximation to actually observed relationships, certainly no one doubts. Experience can however, say nothing more than this, and, thus far, it has not said this with anything like the degree of certainty very generally ascribed to it.

The above mentioned absurdities disappear on the introduction of arbitrarily small, though finite, corrections to Newton's law, which need become appreciable only when vastly extended regions of space are considered. It follows then, that the claim for these corrections needs in no wise, to be legitimized by the results of observation within so small a part of space as the planetary system occupies, for within even vastly more extended regions, the Newtonian law might satisfy the most accurate observations possible. Beyond the planetary system the results of observation do not at all suffice as yet to confirm with certainty any deviations from this law.

Though highly probable, it is far from self-evident, that the forces of attraction follow the same law in all places throughout the universe. In the present state of science we can only suspect with some slight degree of justification that the motions of double stars, for example, are governed by Newton's law in precisely the same way as are the planets. But the degree of closeness, with which known double star motions have confirmed this suspicion is after all quite small. As I have repeatedly and emphatically said, we can only say, that in the double star systems, Newton's law has only in a broadly general way been established. Concerning possible small corrections, which, within our planetary system, would occasion the most intolerable discordances between theory and observation, the relatively inexact double star measures would be wholly non-committal. As to the validity as a rigorously exact formula, of the Newtonian law throughout the vast regions of interstellar space, there does not as yet exist any experimental knowledge whatever.

We can therefore, derive from experience absolutely nothing concerning the inadmissability of a correction to Newton's law, because even within our planetary system, this correction does not exceed an assignable magnitude. It nevertheless seems, that many would ascribe to the law of gravitation the property of an *a priori* result of cognition. The Newtonian formula has indeed, been said by some to be necessarily connected with our idea of space and even, to follow logically from it. Such notions are simply inconceivable to unbiased consideration. In point of fact, a law of force has absolutely no other condition to fulfill than to be a *sufficiently* exact and hence, at best, only an *approximate* representation of the observed motions. This aggregation of facts into a formula is subject only to the self-evident limitation that no inconsistencies may result from it.

But, since the Newtonian law is nothing more than a formula, derived purely empirically, which, within narrowly limited portions of space, furnishes a high degree of approximation to the facts of observation, any other formula, which does the same service may replace it, in case this substitution is recommended by other scientific considerations.

From the foregoing explanation, it follows that a correction to Newton's law is at any rate, desirable even for the reason that if this correction be made, one escapes the danger of erroneous metaphysical considerations, touching the *finity, or infinity* of matter. But this correction seems absolutely necessary, if the assumption be made that cosmic matter is unlimited. This lat-

ter opinion is for many persons self-evident, for others of course, it is not. In my opinion, however, this matter can be decided by ***observation***, neither now, nor in the future, though attempts have been frequently made. Even the hypothesis of an absorption of light in interstellar space, plausible as it is in itself, destroys the hope of ever being able to see luminous bodies beyond certain limited distances. In case interstellar space is strewn with numerous non-luminous bodies, perspective occultations, as also the absorption of light by extensive nebulous areas, which, according to recent observations seem to surround us everywhere, would very much restrict the portion of space accessible to our optical instrument. Direct vision throughout an infinite space is wholly out of the question. Moreover, in the above discussion it is not merely a question of those masses which during a few thousand years happen to be in such condition as to make possible the emission of rays, whose wave-lengths are included within close limits. Masses in widely different conditions are to be entered into the account. That luminosity is not an essential attribute of cosmic matter is a self-evident fact; but it is also proved by experience. Gravitation must, on the contrary, be regarded as essential to matter if we dare not question its universal validity, *a priori*. This point will he more completely discussed in the next section.

The inconsistencies to which Newton's law leads, disappear however, for any law of the potential for which the integrals occurring in (1) have a meaning and similarly, if δ has finite values everywhere throughout space. Such laws evidently exist in ample numbers and no conclusion at all concerning the corrections the law must undergo, could then be drawn from the foregoing considerations. Carl Neumann has investigated this in relation to the electrostatic forces, in the question of the theory of forces operating over distances, and although the results of his investigations are not in their unmodified form, applicable to the phenomena of gravitation, yet the close analogies in other respects, of these two species of force, make the thorough-going investigation of Neumann, of highest significance to the present discussion.

Neumann proposes, in another place, the difficult problem of finding those laws of force, which permit of a condition of equilibrium of electrical matter, distributed over conductors of any sort whatever. He finds that unambiguous conditions of equilibrium, whose existence is regarded as demonstrable experimentally, occur, when the potential of the attractive and the repulsive forces in operation, are given by the formula,

$$f(r) = \frac{Ae^{-\lambda_1 r}}{r} + \frac{Be^{-\lambda_2 r}}{r} + \ldots \tag{4}$$

Where the λ's are positive and the constants A, B, C, etc. all have the same signs.

We see immediately, that this law of the potential removes the aforesaid difficulties, whenever the λ's have finite, though ever so small values, for the expressions (I), represent in this case, finite and determinate magnitudes. In the limiting case, where the assumption is made of an infinitely great number of terms, of such form as to make the right side of (4) a definite integral, the law of the potential is then represented thus:

$$f(r) = \frac{A}{r^{1+\lambda}}. \tag{5}$$

This law has been thoroughly investigated by Green and applied to electrostatic problems. It is of astronomical interest only when λ is an extremely small proper fraction. According to formula (1), it does not remove the difficulties mentioned, for V_1 and X_1 are generally, defined by meaningless expressions in case δ is finite, while, of course, Z_1 preserves a finite value. For this reason, *this law must be set aside as furnishing no advantage over the law of Newton.*

Even as long ago as the time of Newton, it was known that formula (5) produces secular motions in the perihelia of planetary orbits and hence, it is but natural to expect that by an appropriate choice of λ, the well-known anomaly in the perihelion motion of Mercury's orbit, might be explained. Consequently, Mr. A. Hall* has recently proposed this law ($\lambda = 0.00000016$) and Newcomb† declares this proposal to be plausible. For the reasons adduced in the foregoing, I am unable to concur in this view.

I have myself‡ given the formula

$$\frac{mm' \cdot e^{-\lambda r}}{r^2} \tag{6}$$

for the attraction between the two masses m and m', as an example of a law of distance-forces, which removes the objections mentioned above.

As was expressly stated, when this formula was given, it was not intended as a correction to be actually applied to the New-

* *Ast. Jour.* No. 327.
† The Elements of the four inner Planets. Washington '95.
‡ *A. N.* No. 3273.

tonian law. It was only cited as an example, which would correspond to the analogy between the propagation of light and gravitation. This formula was moreover, mentioned by Laplace* who likewise offered it as a supplement to the theory of the propagation of light in absorbing media. That integrals (I) have a meaning for this form is readily apparent.

Whether the formula (4) or (6) is fitted to displace the law of Newton cannot, at present, be decided with generality. In the solar system, with some isolated exceptions, Newton's law has demonstrated its capability to explain the movements, even in detail. Among these exceptions, the peculiarity of Mercury's perihelion motion, is to be classed. Formulæ (4), (5) and (6) all give a motion whose magnitude is easily calculated. The constants in these formulæ, are to be assumed small *a priori*. Putting now, for brevity, $k^2(1+m) = \mu$ where k is the Gaussian constant, m the mass of the planet and 1 is the solar mass, we must assume in formula (4) of Neumann

$$\mu = A + B + \ldots ; \quad \mu\lambda^2 = A\lambda_1^2 + B\lambda_2^2 + \ldots .$$

and we then obtain for the component, acting in the direction of the radius vector, with sufficient accuracy,

$$R = \frac{\mu}{2}\lambda^2.$$

Green's formula (5) gives for R

$$R = \frac{\mu}{r^2}(1 - r^{-\lambda})$$

and finally, Laplace's formula (6) furnishes

$$R = \mu\frac{\lambda}{r}$$

The variations of the elliptic orbital elements; e, the eccentricity; a, the semi-major axis; χ, the longitude of perihelion, are

$$\frac{de}{dv} = \frac{Rr^2 \sin v}{\mu}, \quad \frac{1}{a}\cdot\frac{da}{dv} = \frac{2eRr^2 \sin v}{\mu(1-e^2)}, \quad e\frac{d\chi}{dv} = -\frac{Rr^2 \sin v}{\mu},$$

introducing the true anomaly instead of the time as independent variable. Secular terms arise only in χ. An extremely simple computation gives for the secular change, for the time Δt, if we retain only the first power of e, and put $dv = n \cdot \Delta t$,

for Neumann's formula: $\quad \Delta\chi = \frac{\lambda^2 a^2 n}{2}\Delta t = \frac{\lambda^2\mu}{2}\sqrt{a}\cdot\Delta t,$

* *Mechanique Celeste* t. V. Livre XVI Chap. IV.

for Green's formula: $$= \frac{\lambda n}{2} \Delta t = \frac{\lambda \mu}{2} \cdot \frac{1}{a^{\frac{3}{2}}} \Delta t,$$

for Laplace's formula: $$= \frac{\lambda a n}{2} \Delta t = \frac{\lambda \mu}{2} \cdot \frac{1}{a^{\frac{1}{2}}} \Delta t.$$

The secular changes for the perihelion are then, for the single planets, according to
Neumann's formula proportional to $\sqrt{a}$,
Green's formula proportional to $\left(\frac{1}{\sqrt{a}}\right)^3$,
Laplace's formula proportional to $\frac{1}{\sqrt{a}}$.

If we put $\Delta\chi$ equals 40″ per century for Mercury, which is the unexplained part of the perihelion motion of this planet, we find for

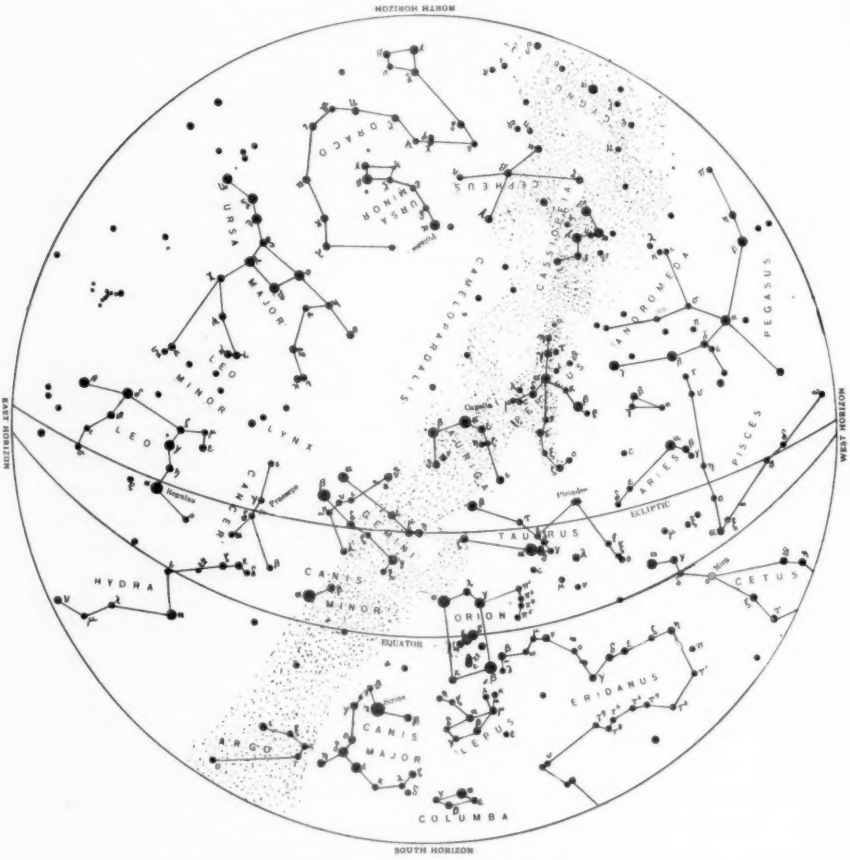
	Neumann	Green	Laplace	Newcomb
Venus	55″	16″	29″	− 8″ ± 37″
Earth	64	10	25	6 ± 8
Mars	79	5	20	8 ± 4

In the column under "Newcomb," are set those values of the perihelion motions, in addition to their mean errors, which Newcomb has derived empirically from observations, and which are not explained by theory. These empirical corrections are certainly not to be explained by Neumann's formula, hardly by Laplace's, but surprisingly well by Green's. Nevertheless, it must be considered accidental, that Green's formula corresponds to the observations so closely. The objections to this formula are to my mind, of greater weight than can be ascribed to the agreement of the above cited numerical values. The anomalous perihelion motion of Mars, which is chiefly instrumental in the decision, does not seem to be any better confirmed, than other numerical values found by Newcomb; for example, that for the motion of the node of Venus' orbit, which it is wholly impossible to represent by a modification of the law of attraction. It is then, not improbable, that the motion of the perihelia of Mercury, of Mars, etc. may find their explanation in quite different and more simple causes than these. In this event, no indication as to the proper clew to the explanation would exist in the planetary system, to point to the necessity of a correction to the law of Newton. Since however, the necessity for an unlimited application of Newton's law is proved in the foregoing, this only signifies that the planetary system is not sufficiently extended to decide the question under consideration.

PLANET NOTES FOR FEBRUARY.

H. C. WILSON.

Mercury is a morning planet just past greatest western elongation, and may be seen toward the southeast about an hour before sunrise. None of the stars in Sagittarius, through which constellation Mercury is moving eastward, are as



THE CONSTELLATIONS AT 9 P. M., FEBRUARY 1, 1898.

bright as the planet so there need be no uncertainty as to which is the planet. On Feb. 11 at noon Mercury will pass close by Mars; at the time of conjunction in right ascension the planets will be less than 1′ apart in declination. Mercury will be the northernmost and the brighter of the two.

Venus will be at superior conjunction, *i. e.* on the farther side of the Sun and in line with the Earth, on February 15. The planet is therefore not in good position for observation, but it may be of interest to some to see how close to the Sun they can follow the planet, first with the naked eye then with a telescope.

Mars is not in favorable position for observation but it will be of interest to note the relative positions, brightness and colors, of Mercury and Mars on the morning of February 11. Possibly some may be able with telescopes to observe the two planets when they are nearest together, a little before noon on that day.

Jupiter will be near the meridian about three o'clock in the morning during February. The planet is but a little south of the equator so that his altitude is quite favorable to good viewing. Jupiter is retrograding slowly in Virgo near the famous double star Gamma. His light is intensely brilliant, far surpassing that of any of the stars in the vicinity.

Saturn does not reach the meridian until between seven and eight o'clock in the morning, but moderately early risers may obtain good views of this glorious planet, which is to be seen toward the southeast in the constellation Scorpio. The golden color of Saturn contrasts finely with the ruddy hue of Antares, and thus adds to the beauty as well as to the brilliancy of the constellation. The rings of Saturn are opened to nearly their greatest width, the elevation of the Earth above the plane of the rings being about 26° during this opposition.

Uranus is also in Scorpio, about 1° east and 1° south of the star Beta, and will move slowly eastward during this month. Uranus will be at quadrature, 90° west from the Sun, on February 21.

Neptune is now in the most convenient position for observation, being near the meridian at eight o'clock in the evening, at an altitude of about 67°. The planet is about 4° west of the star Zeta Tauri and a little north; it is almost directly west of the star Omicron and 1° distant from the latter. Neptune is not brighter than many of the telescopic stars in the vicinity so that it is recognized with difficulty, unless one uses a telescope with sufficient power to reveal the disk of the planet. Anyone may find it with a small telescope by making careful charts of the stars in the vicinity on two or three nights a week apart.

Sunspots.—The sunspot minimum still refuses to consider itself at hand. Several notable outbursts have occurred during the past three months. During January good sized spots were visible on every date when the Sun was observed at Northfield. On Jan. 4 there were four groups, each of three of them containing a large spot. Again on Jan. 21 five groups were noted containing seventeen spots in all, not counting very minute umbra.

Occultations Visible at Washington.

Date 1898.	Star's Name.	Magnitude.	IMMERSION. Washington M. T.	IMMERSION. Angle f'm N pt.	EMERSION. Washington M. T.	EMERSION. Angle f'm N pt.	Duration.
			h m	°	h m	°	h m
Feb. 5	o^2 Cancri	4.0	10 43	116	12 9	307	1 26
5	o^1 Cancri	5.7	11 21	189	11 53	232	0 32
6	10 Sextantis	6.0	18 7	107	19 2	307	0 55
7	34 Sextantis	6.7	17 30	94	18 27	326	0 57
13	A^2 Scorpii	5.2	13 44	104	14 48	296	1 4
13	3 Scorpii	6.7	14 15	67	15 4	333	0 49
25	26 Arietis	6.0	7 44	84	8 56	250	1 12

The Moon.

		Phases.	Rises. (Central Standard time at Northfield; Local Time 13m less)		Sets.	
			h	m	h	m
Feb.	6	Full Moon	5	44 P. M.	7	38 A. M.
	13	Last Quarter	12	55 A. M.	10	5 "
	20	New Moon	6	48 "	6	3 P. M.
	28	First Quarter	9	44 "	1	57 A. M.

VARIABLE STARS.

I. A. PARKHURST.

Minima of the Variable Stars of the Algol Type.

[Given to the nearest hour in Greenwich Mean Time.]

1898.

U CEPHEI.

	d	h
Mar.	2	20
	5	8
	7	20
	10	7
	12	19
	15	7
	17	19
	20	7
	22	18
	27	18

ALGOL.

Mar.	4	9
	18	17
	21	14
	24	11
	27	7

λ TAURI.

Mar.	26	16
	30	15

R CANIS MAJORIS.

	d	h
Every 8th min. $P = 1^d\ 3^h.3$		
Mar.	2	15
	11	17
	20	19
	29	22

S CANCRI.

Mar.	15	10

S VELORUM.

Mar.	1	16
	7	15
	13	13
	19	11
	25	10
	31	8

δ LIBRAE.

Mar.	1	13
	3	21
	8	13
	10	20
	15	12
	17	20

δ LIBRAE Cont.

	d	h
Mar.	22	12
	24	20
	29	11
	31	19

U CORONÆ.

Mar.	9	23
	16	20
	30	16

U OPHIUCHI.

Every 10th min. $P = 20^h.1$		
Mar.	2	16
	11	1
	19	10
	27	20

RS SAGITTARI

Mar.	2	13
	4	22
	14	14
	21	20
	26	16

W DELPHINI.

	d	h
Mar.	8	15
	27	20

Y CYGNI.

Even minima.		
Mar.	0	15
	3	15
	6	15
	9	15
	12	15
	15	15
	18	15
	21	15
	24	15
	27	15
	30	14
Odd minima.		
Mar.	1	17
	4	17
	7	16
	10	16
	13	16
	16	16
	19	16
	22	16
	25	16
	28	16
	31	16

Y CYGNI. Since the above ephemeris was printed Duner has given a set of revised elements in *Astronomische Nachrichten*, No. 3467, according to which the even minima occur 0.8 hours earlier in March and the odd, 1.5 hours later.

The following ephemeris of Long Period Variables, taken from the *Astronomical Journal* No. 420, differs in two particulars from those previouslygiven. In the column headed "Mag." is given the expected magnitude of the star at the phase indicated. This will be a great help in selecting an observing list for the aperture available. In the second place the dates are left vacant for many of the minima, which are expected to occur some time during the month. As it is of especial importance that as many minima as possible should be observed, these approximate times will be of assistance in arranging an observing list.

Maxima and Minima of Long Period Variables.

1898.

(From Dr. S. C. Chandler's ephemeris in *Ast. Jour.* No. 420).

MAXIMA.

		Mag.	Feb.
401	U Sculptoris	8.5	19:
434	S Piscium	8.5	10
976	T Arietis	8	5
1386	T Eridani	7	25
1582	S Tauri	9.5	28
1855	R Aurigæ	7	27
1894	T Columbæ	7.5	13
2258	V Aurigæ	9	3
4511	T Ursæ Maj.	7	7
5338	U Bootis	9 5	3
5494	S Libræ	8	15
5593	W Libræ	10	21
5776	X Scorpii	10	3
5902	Y Scorpii	10	9
5955	R Draconis	7.5	8
6132	R Ophiuchi	7.5	18
6207	Z Ophiuchi	8	9
7085	RT Cygni	7	15
7252	W Capricorni	10.5	10
7404	R Microscopii	8	22
7455	U Capricorni	10.5	15
7733	Y Capricorni	10.5	1
7909	S Piscis aus.	9	19

MINIMA.

		Mag.	Feb.
62	S Sculptoris	10	—
112	R Andromedæ	< 13	27:
715	S Arietis	14	24:
782	R Arietis	12	2
1717	V Tauri	< 13	2
1981	S Camelop.	11	10
2266	V Monocerotis	11	—
2583	L_2 Puppis	6	3
3184	T Hydræ	< 13	—
3477	R Leonis min.	13	23
3994	S Leonis	< 13	2:
4315	R Comæ	< 13	—
4377	T Virginis	12	—
4940	W Hydræ	8	—
5037	RR Virginis	< 14	—
5644	Z Libræ	< 13	—
5831	S Scorpii	< 13	—
5931	S Ophiuchi	< 13	—
6170	RW Scorpii	14	—
7659	T Capricorni	13.5	22

MAXIMA.

		Mag.	Mar.
1850	S Pictoris	8.5	19:
2100	U Orionis	7	28
2625	V Geminorum	8 5	29
2815	U Geminorum	9.3	29:
2976	V Cancri	7	15
3264	W Cancri	9.5	12:
4492	Y Virginis	8.5	1
4521	R Virginis	7	8
4596	U Virginis	8	21
5095	R Centauri	6	14
5237	R Bootis	7	13
5321	S Lupi	9.5	12:
5504	S Coronæ	7	8
6608	RV Sagittarii	8	26
6894	S Lyræ	9	31:
6905	R Sagittarii	7.5	19
6943	T Sagittæ	8 5	13:
7139	RR Sagittarii	7.5	19
7220	S Cygni	10	19
7448	W Aquarii	8	5
7571	V Capricorni	9	17
7907	U Aquarii	9 5	12:
8230	S Aquarii	8.5	31
8597	V Ceti	9	27

MINIMA.

		Mag.	Mar.
466	U Piscium	15	26
659	X Cassiopeæ	12	23:
678	U Persei*	11 5	16:
893	U Ceti	12	3:
1222	R Persei	13	26
1771	R Leporis	} 8.5	23
2478	R Lyncis	< 13	30
2530	V Can. min.	< 13	—
2684	S Can. min.	11	11
3244	S Pyxidis	11	—
3493	R Leonis	10	26
3637	S Carinæ	9	4
4816	V Virginis	< 13	—
5190	R Camelopard.	12.5	28:
5430	T Libræ	< 14	25
5761	Z Scorpii	12	—
5770	R Herculis	< 13	—
6512	T Herculis	11	21
6900	W Aquilæ	11	—
6903	T Sagittarii	< 11	—
7242	S Aquilæ	11	15
7260	Z Aquilæ	11	—
8068	S Lacertæ	< 12	31
7560	R Vulpeculæ	13	14

* The above minimum of 678 U Persei is given in Chandler's ephemeris, by a typographical error, as 678 U *Piscium.*

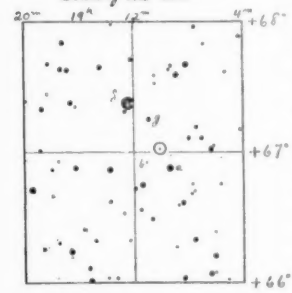
NOTES.

ANDERSON'S VARIABLE IN COMA, R. A. 12h 23m 1s, Decl. + 32° 18′ 19″ (1855) which was charted in the September number, page 270, has changed enough to make its variability certain. It increased from 11.7 mag. 1897 Sept. 3, to 9.5 mag. Dec. 31.

In *Astronomical Journal* No. 424 this star is given the designation 4471 T Canum Venaticorum, by Dr. Chandler. It lies near the boundary of the constellations Coma and Canes.

THREE NEW VARIABLES DISCOVERED BY ANDERSON. In *Astronomische Nachrichten* Nos. 3461 and 3463 he announces the variability of the stars:

DM. + 30°,1329	R. A.	6h	37m	50s	Decl. + 30°	25′.2	(1855)
DM. + 67 ,1124		19	9	54	+ 67	2 .4	"
Anon.		19	31	10	+ 11	23	"



The second star is in the constellation Draco, ½ degree south preceding the third magnitude star δ Draconis, making it very easy to find and identify. Its circumpolar position renders its observation possible throughout the year. A small chart is therefore given of the region, on the scale of the Durchmusterung, showing in addition to the DM stars, the stars *b* (9.7 mag. according to Anderson) and *e*, only the former being lettered on the chart. Nov. 26, 1897, Anderson found the variable 0.3 mag. brighter than *b*. Jan. 4 and 5 I found it 0.3 or 0.4 mag. fainter than *b*. The co-ordinates and magnitudes of a few of the comparison stars are as follows:

	Co-or. from the variable. R. A.	Decl.	Mag.		Co-or. from the variable. R. A.	Decl.	Mag.
	s	′			s	′	
e − 60	− 5.9	− 3.4	11	*d* − 14	− 1.4	+ 1.4	11
a − 51	− 5.0	− 9.2	7.9	*b* + 60	+ 5.9	− 7.0	9.7
f − 40	− 3.9	− 8.3	11	*g* + 54	+ 5.4	+ 14.4	8.8

U GEMINORUM. Thanks to the industry of Messrs. Sperra and Daniel, we have a fair determination of the November maximum. Their observed magnitudes are as follows, in Greenwich time:

	Gr. Time.	Mag.	
1897 Nov.	21.97	< 12	D
	23.97	9.9	S
	24.64	9.2	D
	27.73	10.1	D
	27.97	9.9	S
	28.63	10.4	D

The maximum evidently occurred about Nov. 24. This is the fourth maximum observed in 1897. I can find no record of more than three in any previous year.

SS CYGNI. From the same two observers we have the following estimates of SS Cygni:

		Gr. Time.	Mag.	
1897	Nov.	28.63	11.3	D
		30.50	9.4	D
	Dec.	7.52	9.6	S
		8.49	10.1	S
		9.47	10.1	D
		10.54	10.5	D

While these observations are not sufficient to determine the time of maximum, they fix the sudden rise at Nov. 30, or 59 days after the rise at the October maximum.

Note.—The omissions, lateness and disjointed condition of some of the above notes are due to the irregular time of appearance of the last number of this publication, and in no way the fault of Mr. Parkhurst. EDITOR.

COMET NOTE.

Rediscovery of Winnecke's Comet.—This comet, the ephemeris for whose return was given in our December number was found by Perrine with the 36-inch telescope at Lick Observatory on the morning of January 2. The observed position was Jan. 2.0873 R. A. = $15^h\ 19^m\ 2^s.5$; Decl. = $-3°\ 58'\ 34''$.

The corrections to the ephemeris indicated by this observation are $\Delta\alpha = +1^m\ 58^s$ and $\Delta\delta = -8'.6$ The comet is very faint.

Later observations by Mr. Perrine are given in No. 424 of the *Astronomical Journal*. Dr. Chandler, correcting Von Hærdtl's elements by means of Perrine's observations obtains the following:

ELEMENTS.

$T = 1898$ Mar. 20.392 Gr. M. T.
$\omega = 173°\ 21'\ 10''.0$
$\Omega = 100\ 51\ 45\ .5$ } 1898.0
$i = 16\ 59\ 34\ .0$
$\log e = 9.854161$
$\log a = 0.510521$

A portion of his ephemeris based upon these elements is as follows:

	App R. A.	App. Decl.	log Δ
Jan. 30.5	$17^h\ 15^m\ 38^s.03$	$-10°\ 29'\ 41''.8$	0.1608
Feb. 1.5	25 1.63	−10 53 56 .4	
3.5	34 33.95	−11 17 21 .0	0.1527
5.5	44 14.66	−11 39 50 .2	
7.5	17 54 3.34	−12 1 18 .5	0.1457
9.5	18 4 0.23	21 40 .1	
11.5	14 2.88	40 49 .7	0.1400
13.5	24 12.71	−12 58 42 .2	
15.5	34 28.36	−13 15 12 .5	0.1356
17.5	44 49.18	30 15 .4	
19.5	18 55 14.35	43 47 .1	0.1326
21.5	19 5 43.16	−13 55 43 .1	
23.5	16 14.76	−14 6 0 .8	0.1310
25.5	26 48.40	14 36 .4	
27.5	37 23.22	21 28 .2	0.1307

GENERAL NOTES.

Volume V of this publication closes with this number. Subscriptions ending with the volume should be renewed promptly in order to insure continuance. An unusually large number of renewals for volume VI have already been received. The outlook for 1898 is encouraging.

Subscribers are reminded that volume VI begins with March number and ends with December 1898, and contains 10 numbers as usual. This publication will be issued every month during this calendar year. Subscriptions for the year can be secured if desired at a cost of $3 00 for the 12 numbers.

Mathematical Articles.—Because of the importance of the matter treated, we have admitted into this issue several articles using the mathematics freely. It is to be hoped that this feature will not discourage popular readers. It ought not to do so. In connection with most of the equations there will be found popular statements of the results reached by the mathematics used. The popular reader needs to know these results. Now if such readers will go through the matter neglecting the mathematical formulæ which they do not understand, they will certainly get the points aimed at by the writers, which are the most important things to be learned. If methods by which results are obtained are long or difficult, most people must trust the scholars to do that part of the work. For all who have interest in useful results have neither time nor ability to prove everything in the range of needed knowledge.

Measures of Sirius, Procyon and β 395.—In *Astronomical Journal* Mr. R. G. Aitken gives measures of these stars with the 36-inch telescope of Lick Observatory. The results of 4 nights' measures of Sirius and 3 of each of the other stars are as follows:

	Date.	Position Angle.	Distance.	
		°		
Sirius	1897.81	174.7	4.03	4*n*
Procyon	1897.88	323.8	4.70	3*n*
β 395	1897.92	273.7	0.27	3*n*

New Asteroids.—Three new asteroids were discovered on Dec. 18 by Charlois at Nice, France. They were all within an area of less than three degrees diameter.

	Mag.	Nice M. T.			R. A.			Decl.		Daily Motion.	
			h	m	h	m	s	°	′	°	′
DM	12	1897 Dec. 18	9	29.0	4	42	4	+ 18	0	− 48	− 10
DN	12.8	18	9	29.0	4	47	0	+ 20	44	− 48	− 1
DO	12	18	9	29.0	4	53	28	+ 19	21	− 56	0

J. M. Hadley, of Flower Observatory, University of Pennsylvania, Philadelphia, requests that any unpublished observations of Comet g 1896 (VII) discovered by Mr. C. D. Perrine be made public or sent to him at an early date.

Brilliant Meteor. Monday, November twenty-ninth, at ten minutes past eight in the evening, three people saw a large meteor. It was circular, fully one-fourth the apparent size of the full moon, at first white, changing to a flaming red as it vanished, leaving a bright red, green and blue train.

The path of the meteor seemed to be from the zenith, about the position of the θ (theta) nebula in Andromeda, down toward the western horizon. The observers were going west and one said the meteor went northwest, one west, and the other southwest.

It passed south of Delphinus or "Job's Coffin," and vanished at about the upper northern edge of Aquarius. Total time to the disappearance of the train was about ten seconds. Total length of path, zenith to Aquarius, 60° to 70°. It

appeared to be three-fourths of the distance from the zenith to the horizon. The night was clear, with no perceptible haze. Latitude, 40° 6′ longitude, 89°.

Urbana, Ill., Dec. 23, 1897. MARION E. SPARKS.

Largest Telescope Reflector.—The December *Popular Science News* reports that another great telescope is already approaching completion: In 1895, the Rev. D. John Peate commenced, at Greenville, Pa., the construction of a mammoth speculum for a refleting telescope designed for the American University at Washington. After two years of arduous toil, Dr. Peate has given the great mirror its last touches—*i. e.*, in grinding and polishing, and it is ready for silvering, and for the futher and very important stage of mounting. The huge disc of glass is more than 61 inches in diameter.

Variables of the Algol Type. Dr. T. J. J. See has arrived at the result that the orbits of binary stars exhibit a tendency towards an eccentricity of about 0.5 consequently instead of assuming a circular orbit in the case of eclipse-variables we should, I apprehend, assume one with about this eccentricity. Without a third body in the system there might be no reason for a change in the position of the periastron similar to our "precession of the equinoxes." Your mathematical readers may perhaps take up the question, but it strikes me that such a precessional change might account for the small differences observed in the period.

I may however notice two other possible explanations. 1. Gravity is propogated in time and the effect of the time-propagation is greatest when the angular movement of the satellite is most rapid. 2. The orbit of the satellite, which eclipses the bright star periodically, is altering rapidly under the influence of tidal evolution—the pair of stars being comparatively close and both being in a liquid or gaseous condition. Careful observations ought to show in a comparatively short time whether either or both of these causes are at work. How far these two causes might produce identical results I am not competent to say, but I hope some of your able contributors will analyze the problem. How would a time propagation of gravity affect an eclipse-star of short period? W. H. S. MONCK.

Dublin, Ireland.

New Paraboloid Speculum.—I observe the interesting nocite of the new diagonal paraboloid speculum devised by Dr. Lane Poor of Johns Hopkins University in last number of POPULAR ASTRONOMY upon which I would like to offer a few remarks.

At Dr. Poor's request I visited Johns Hopkins University in the early part of this year, among other things, for the purpose of assisting him in getting out a mirror of the description named in your paragraph on page 502, that is to say one that should be capable of depicting a regular optical image of an object situated at an angle of 90° to the line of vision, altho' nearly 25 years ago in the "*Engl-h Mechanic*," and more recently in "*Design and Work*" I had suggested some such a mirror. I never knew until my attention was called to an article in an English periodical 10 or 12 years ago, that any such mirror had been attempted—and I must say too, that the good Dr. was very reticent in regard to the purpose for which he intended to use the proposed mirror, leading me rather to believe that it was for something connected with photography; nor was it until some progress had been made in working the speculum that I was fully informed of the use to which it was to be put.

I found him working upon a circular disk of glass about 4 inches diameter with a small home-made apparatus constructed upon the model of the late Dr. Draper's well-known speculum grinding machine.

After fully understanding the purpose for which he proposed to use the mirror I suggested that it would be advisable to devise a more adequate and substantial machine, better calculated to assist us in working the required curve or curves.

While the new machine was being built, I proceeded to work the original glass entirely by hand, to as near the proper curves as possible, using the guages already prepared by Dr. Poor, but found it to be an extremely tedious and unsatisfactory job, the curves in some places coming out more than an inch from the intended radii.

When the machine designed by Dr. Poor had been placed *in situ*, a number of very perfectly turned and ground guages were prepared; for the proposed grinding tool and polisher being in shape, an oblong of about 6 inches by 4, the machine was intended to be used principally as a guide and labor saver, all the curves having to be very carefully guaged at short intervals.

The result of a few days' very hard work was a mirror with curves along the major axis of a radius of about 84.1 to 84.5 inches while those across the minor axis varied from about 42 to 44 inches.

The polishing, as I anticipated, was a long and tedious process, and the ultimate result not wholly satisfactory from an astronomical point of view. Upon testing it at the various radial centers it was shown to deviate in some places several tenths of an inch from the calculated radii, yet upon placing it in position upon a temporary stand and applying an eyepiece giving a power of about 30 diameters or more, I believe it was a one-inch eyepiece we used, it afforded us a very plain and pleasing view of people walking on the top of the Washington monument some half mile distant from the Observatory, indeed their features, with the above named power, were shown quite distinctly enough to have ensured the recognition of any familiar face.

The result of this experiment led me to somewhat modify my preconceived opinion of the possible usefulness of such a mirror when properly made and mounted—for I had at first uncompromisingly condemned and ridiculed the idea—at this juncture being compelled by the exigencies of business to return home, I carried the experiment no farther, but subsequently wrote Dr. Poor, enclosing him a diagram of a machine that was certainly simpler than the one we had been using, and by means of which I believe the curves of such a mirror might be worked out almost automatically with tolerable correctness, the finishing touches being readily given by hand.

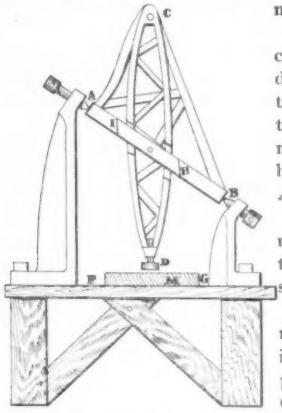
Although I have only heard indirectly, I understand that the result has been encouraging, and that Dr. Poor is satisfied with the progress he has made, but whether by the use of my machine or no, I have not learned, probably Dr. Poor will enlighten us in this regard.

After all, the difficulties to be surmounted in making a mirror of this description of large aperture would, in my estimation, be immense, and probably altogether incommensurate with the advantages to be derived from it.

Not only is the labor of working such a surface, with precision enough to enable it to stand a high power, very great, arising partly from the fact of the much larger area required than that of a circular mirror of equal aperture, in the proportion of about 3 to 2; but the trouble of testing it, and more particularly of placing it in position for testing, the least tilting or twisting, which would be of no consequence whatever in testing a direct vision mirror, being of vast consequence in a mirror of this kind.

The tediousness of the work is also greatly increased by the necessity for frequent testings at short intervals, and the difficulty of precisely locating the parts that need rubbing down by local touches, difficulties only to be appreciated by those who who are familiar with the work of grinding and polishing large optical surfaces.

But I believe that with carefully calculated curves and a well-made machine on the lines of the accompanying diagram, which is a copy of the one I sent Dr. Poor, a practically perfect mirror of 10 or 12 inches aperture might be constructed, which would be qulte large enough to test its practicability and usefulness.



The diagram hardly needs explanation. The curves along the major axis of the mirror *M* are derived from the center *C* from which depends the trussed arm *CD* carrying at its lower extremity the grinding tool or polisher *D*. This arm is to be made double, passing on each side and closely embracing the planed part *IH* of the inclined axis *AB*.

The centers of the transverse curves of the mirror lie along the axis *AB*, and the radii of these curves consequently vary as the frame *CD* swings from side to side.

The grinding tools and polishers *D* must be necessarily of small area, which makes the grinding and polishing of the mirror a very tedious process, *C* to *M* about 7 feet, *O* to *M* about 3 ft. 6 in.

In the application of such a mirror to telescopy the chief difficulty would seem to be like that of the dyalitic achromatic, with the oblique rays, in other words, the available field of view must be very contracted. E. M. TYDEMAN.

Camden, N. J., Dec. 24, 1897.

Eclipse of the Sun Jan. 22 —By the papers of Jan. 23 it is reported that the observations of the total eclipse of the Sun on Jan. 22 in India were successful. It is said that the views of the corona were very favorable.

At Talni, India, the observations by E. W. Maunder and C. T. H. Waites were most successful; the sky being perfectly clear with light at the middle of totality about equal to that of the full moon. The general shape of the outlines of the corona was like that of 1886 and 1896, extending over two diameters of the Sun in the direction of the solar equator. Photographs of the corona were secured. The spectrum of the chromosphere and the prominences were also taken. The flash spectrum of the reversing layer was likewise photographed by the aid of a prismatic camera attached to a six inch telescope.

The report from Professor Campbell's Lick Observatory party is essentially the same as the above as far as it goes. So far as learned it appears that the two minutes and a few seconds of totality of the eclipse in India, at least, gave an excellent opportunity to get needed observations of certain solar phenomena very much desired. Reports from the numerous other observing parties have not yet reached us as we go to press for this month.

Errata.—No. 49, p. 601, line 37, for $\log \frac{S}{\sin 1''}$ read $\log S \sin 1''$

Page 369, fifth line from top for distances read distance; 23rd line from top, for Moon's read Moon.

BOOK NOTICE.

Higher Arithmetic, by W. W. Beeman, Professor of Mathematics in the University of Michigan, and D. E. Smith, Professor of Mathematics in the Michigan State Normal College. Published by Messrs. Ginn & Co., 1897, pp. 193.

This new higher arithmetic is intended for high schools, academies and normal schools, in which the subject is supposed to be taken up for a second time with a desire to review and extend the knowledge of this branch previously acquired.

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New Astronomy for Beginners by Professor D. P. Todd, of Amherst College. Publishers, The American Book Company, Chicago, Cincinnati and New York.

Observational Astronomy, revised and enlarged, by Arthur Mee, F. R A. S. Price 60 cents.

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